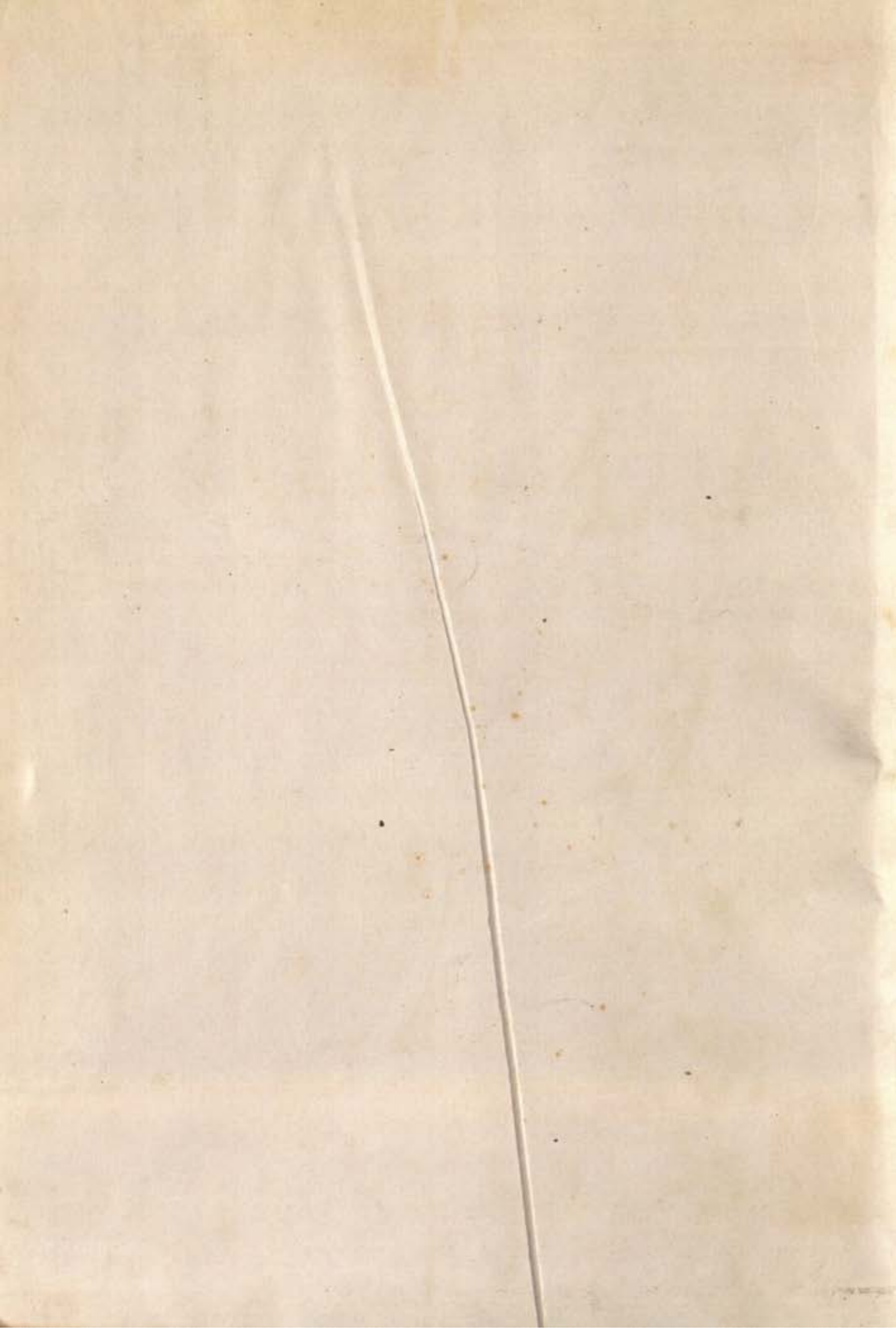


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THE JOURNAL OF Egyptian Archaeology

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(so styled since 1919) was founded in 1882, and incorporated in 1888 as the 'Egypt Exploration Fund'.

Ever since its foundation it has made surveys and conducted explorations and excavations in Egypt, in accordance with the best methods of scientific investigation, for the purpose of obtaining information about the ancient history, religion, arts, literature, and ethnology of that country. The Society's activities have recently been extended to the exploration of sites of the Pharaonic Period in the Sudan.

Those of the antiquities discovered which are not retained, according to law, by the Antiquities Departments of Egypt and the Sudan are exhibited in London and are then distributed among public museums in the United Kingdom, the British Dominions, the United States of America, and elsewhere, in strict proportion to the contributions from each locality.

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AN UNNAMED STATUETTE IN SERPENTINE

THE JOURNAL OF Egyptian Archaeology

10599

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EDITORIAL FOREWORD

IN the Foreword to the last volume of the *Journal* it was stated that the Society was proposing, with the consent of the Department of Antiquities of the Sudan Government, to explore the region west of Wādi Ḥalfa, though it had been hoped that a return to Saḫḫārah might also ultimately have been possible. This latter hope was not realized, so that the whole season from November to March was devoted to exploring the site of ancient Buhen, on the west bank of the Nile opposite Wādi Ḥalfa, with the happy result that Professor W. B. Emery, assisted by Mr. D. M. Dixon, has opened up a site which appears to have endless possibilities.

Apart from a few Egyptian expeditions from the First Dynasty onwards, the history of Buhen opens in the Twelfth Dynasty, when there was built there a fortress guarding the north end of the Second Cataract, as a member of the string of fortresses erected at this time to guard the southern frontier of Egypt, which during the Middle Kingdom was regarded as including the Second Cataract. In the times of trouble and disturbance which followed the Hyksos invasion of Egypt proper, the fortress and the town it enclosed seem to have been stormed and sacked, and at the beginning of the War of Liberation in the Seventeenth Dynasty all Lower Nubia as far north as the First Cataract was under the sway of a Cushite ruler. Following on the recovery of Nubia by the earlier kings of the Eighteenth Dynasty, Buhen was reoccupied and the fortifications rebuilt. A fine temple was erected by Queen Ḥatshepsut, and the old Middle Kingdom fortress became a citadel about which grew up a much larger town with its perimeter protected by a fortified wall and dry ditch on a new principle of military architecture consisting of irregular rectangular salients with projecting towers, the nature of which was established by a test clearance of a small area at the extreme north end of the fortress. This disclosed that the main wall, 17 ft. thick, was originally 32 ft. high, while the outer ditch was $22\frac{1}{2}$ ft. wide by 11 ft. deep, revetted with brick and stone. This enlarged town seems to have remained occupied and secure until the final collapse of Egyptian power in Nubia after the end of the Twentieth Dynasty.

The main work of the season, however, was directed to the Middle Kingdom fortifications of the citadel. Professor Emery writes:

Here we found the main walls $16\frac{1}{2}$ ft. thick which had been retained during the later period but which had been strengthened by the construction of large exterior buttresses. At the base of these walls, a wide brick-paved terrace had been built, beyond which was what appeared to be a sunk roadway. As excavation progressed, it became obvious that this sunk roadway covered and followed the original dry ditch of the Middle Kingdom fortress; consequently everything below it has lain undisturbed since 1500 B.C. The removal of part of the terrace and roadway revealed the outer defences of the original fortress, consisting of a rampart with its loopholed parapet overhanging the scarp of the rock-cut ditch. The counter-scarp on the other side of the ditch was heightened by brickwork, surmounted by a narrow covered way with what appears to be the top of a glacis behind it. Projecting from the scarp at intervals are round bastions with double rows of loopholes arranged

in groups of three centring on one single shooting-embrasure from which the defending archer could direct his fire at three different angles downward on to the attackers in the ditch. Some conception of the immense strength of these defences becomes obvious when we realize that an attacking force must first storm the glacis, destroying any outposts concealed in the covered way, while under fire from sling-shots and arrows directed from the main wall above. They would then have to descend the steep counter-scarp, 19½ ft. deep, to the foot of the ditch under an intense and organized cross-fire from the loopholed ramparts and bastions behind which the defenders would be completely concealed. Should they survive this ordeal, they would then have to storm the scarp and rampart above it, only to find themselves in a narrow corridor at the foot of the main walls, which were at least 33 ft. high.

The splendid preservation of the small portion of the fortifications so far cleared is due to the fact that they were deliberately buried to form the foundations of later constructions, and it is clear that our beliefs as regards Egyptian military architecture are about to undergo drastic revision. Small test excavations suggest that the town enclosed by these walls may be equally well preserved, and it appears that the Society's expedition has hit on a site which promises to be exceptionally fruitful and informative. Under the terms of our agreement with the Sudan Government, the preliminary report on which the above account is based will appear in fuller detail in the Sudan Antiquities Department's journal *Kush*, but a brief account with photographs will be found in the *Illustrated London News* for 21 June 1958.

It is with great regret that we have to record the death on 25 October 1957 of Hugh Macilwain Last, who from 1936 to 1949 was Camden Professor of Ancient History in the University of Oxford, and from 1949 until his retirement in 1956 was Principal of Brasenose College. Although his main interest was Roman History, he became a member of the Society in 1920; for a short while prior to 1935 Mr. Last served as Hon. Secretary or Joint Hon. Secretary, but in that year he became Hon. Treasurer, an office which he held until 1948, when he resigned it on his appointment to Brasenose. When Mr. Last first undertook the Hon. Treasurership the Society's finances were at a low ebb, but during his term of office he proved a tower of strength, and it was largely through his efforts that in 1948 we received the Treasury Grant which alone has enabled us to perform our manifest but costly duty to excavate and publish. When he eventually resigned his Treasurership he left the Society's financial affairs in a much sounder condition than he found them.

We have also to deplore the death of Professor Gustave Lefebvre, the eminent French scholar who did so much to train the younger French Egyptologists who are doing such good work today. His most important publications were his *Histoire des grands-prêtres d'Amon Romê-Roÿ et Amenhotep*, *Le Tombeau de Petosiris*, *Grammaire égyptienne*, and *Tableau des parties du corps humain*. Since the above was set up we have also heard of the death on 13 July 1958 of Lady Hopkin Morris, who for nearly twenty years most ably served as our Hon. Secretary, and who is greatly mourned by all who knew her.

The Secretary of the Griffith Institute has asked us to make it known to members that the late Mr. Guy Brunton's drawings of button seal-amulets (63 cards bearing 1027 drawings) and some MS. notes are available in the Institute for study and research.

AN EARLY STATUETTE IN SERPENTINE

By M. CASSIRER

AMONG those Egyptologists who take an interest in the archaeology of Ancient Egypt the complaint is often voiced that, though there are now a not inconsiderable number of books on objets d'art, too many of these are content to reproduce, almost *ad nauseam*, the well-known masterpieces of the big museums. Not only so, but the same illustrations tend to turn up with monotonous regularity. Consequently, many lesser but nevertheless delightful specimens, often tucked away in private collections, but perhaps on the whole even more typical of that amazing culture, have suffered serious neglect to the detriment of the scholar no less than of the aesthete. The superior attitude of the savant—who, in the early stages of our science, it will be remembered, refused to consider any monument not inscribed with an authenticating cartouche—has resulted in our comparative ignorance concerning whole classes of such objects.

The high standard often to be found in the portrait statues of priests and officials is exemplified in a black stone figure of a standing man (pl. I), which was bought in a London shop in February 1957. It is 8½ in. high. It has suffered only insignificant damage. The attitude is the usual one, with the arms at both sides attached to the body. The hands are holding the traditional 'shamstaves'.

The man is stepping out with his left leg, and the base, which is rough on the under-side, extends for over an inch beyond the toes, the nails of which are indicated. There are a few scratches on the base, but no inscription. The modelling of the body is vigorous, emphasizing muscles and breast, with a vertical division from chest to navel above the plain, unadorned belt of the kilt. The latter is short, and folded from left to right on the top and in the opposite direction below. These two parts are perpendicularly pleated, whereas the middle piece, extending further down, has horizontal creases. The head is interesting for the head-dress, which is of the short type that has been aptly described as 'arranged radially, like roof-tiles'. The workmanship is here somewhat less careful in the part invisible in the frontal view, an observation which does not, however, generally apply to the object. The tapering plinth on the back, for instance, is meticulously carved and beautifully polished; it extends nearly to the shoulders. The material is apparently serpentine, which takes a fine polish.

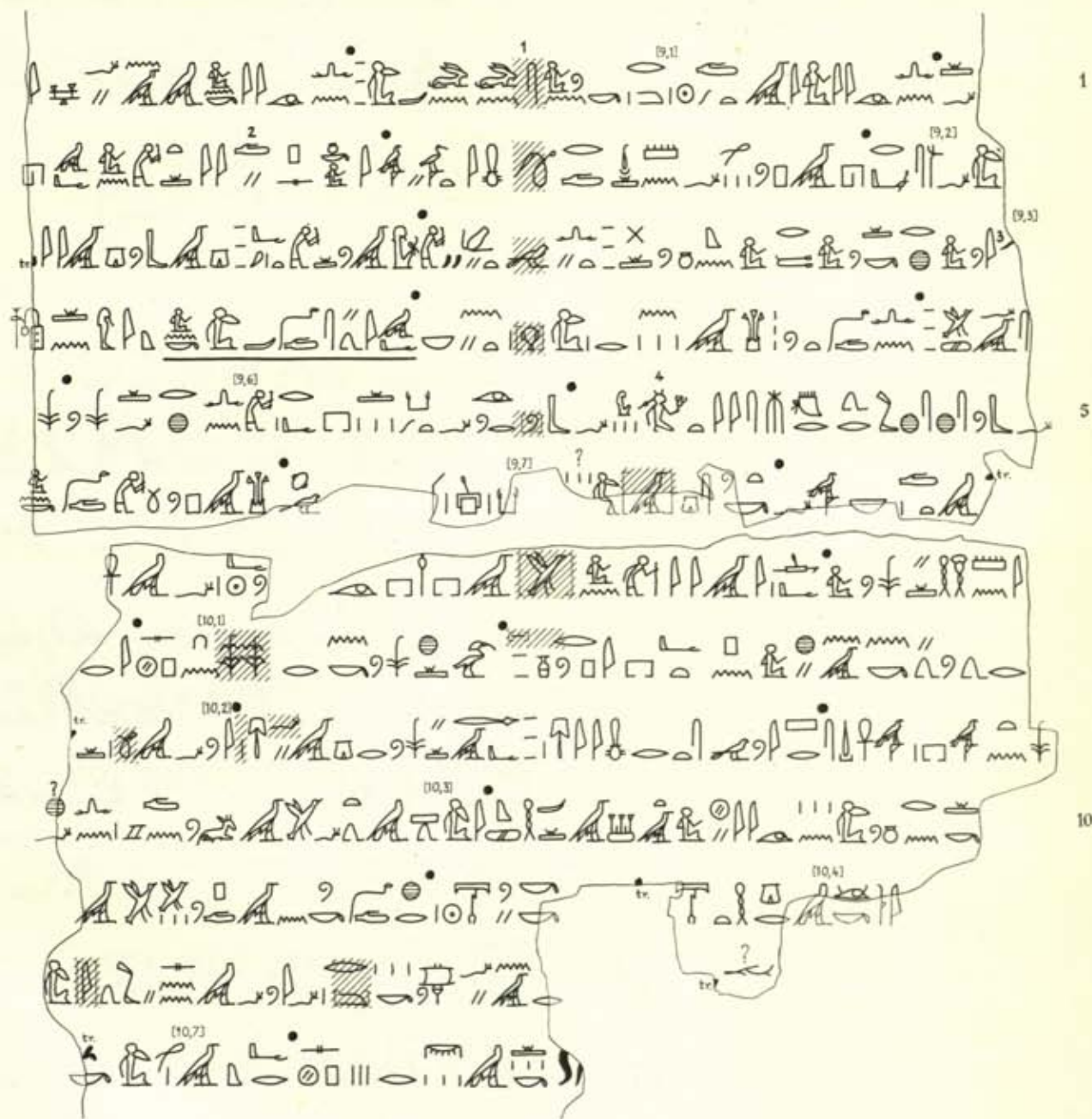
The precise dating presents some difficulty, though sculptures in the same style are found in a few collections. A rather similar one, if slightly bigger, in the Walters Art Gallery is described in Steindorff's catalogue as of black granite and dated Middle Kingdom; it is said to have come from 'Qena'.¹ The figure now numbered 32187 at the British Museum, which is on view, seems to be one of a group of 14, anciently assigned by Budge² to the 'VIth–XIIIth dynasties'. This dating should be sufficiently

¹ Steindorff, *Cat. of . . . Walters Art Gallery* (p. 26), suggests 'Dendera' with a question-mark.

² *B.M. Guide to 4th, 5th & 6th Egyptian Rooms*, p. 119.

elastic to include the actual period of its manufacture. Acquired from a dealer, it is said to have come from Abydos.¹ It is of the same material as the statuette under discussion and, though slightly smaller and less carefully worked, clearly of the same type. It is at present ascribed to the early Middle Kingdom, though the arguments in favour of this dating are, admittedly, not entirely conclusive. The object here published more clearly reflects the forceful, dynamic impact of the Old Kingdom, and it may well belong to its closing phases.

¹ This information is due to Mr. T. G. H. James. Aldred's attempt to distinguish between an Upper and a Lower Egyptian style of sculpture in his *Middle Kingdom Art in Ancient Egypt* (Tiranti, 1950) should be noted in view of the possibility of a later dating than Dyn. VI.



NOTES

1. The papyrus here mounted too close; so too in the following lines.
2. \circ small like \circ to match ω beneath.
3. A small slanting stroke.
4. Possibly intended to be holding a sistrum.

PAPYRUS FRAGMENTS IN THE TURIN MUSEUM

(= An. I, 8, 8-10, 7)

1
[21,2]
[21,3]
[21,4]
10

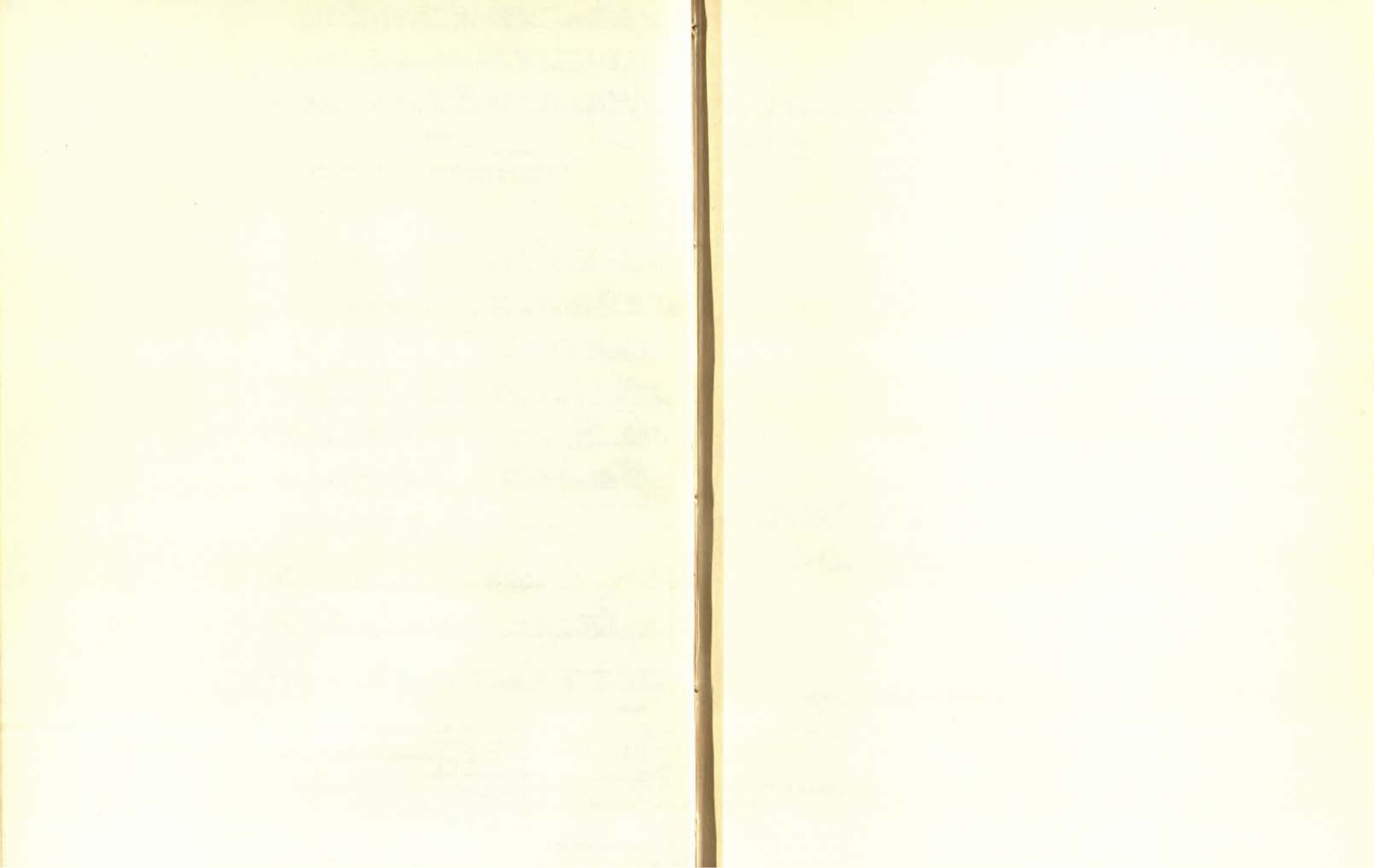
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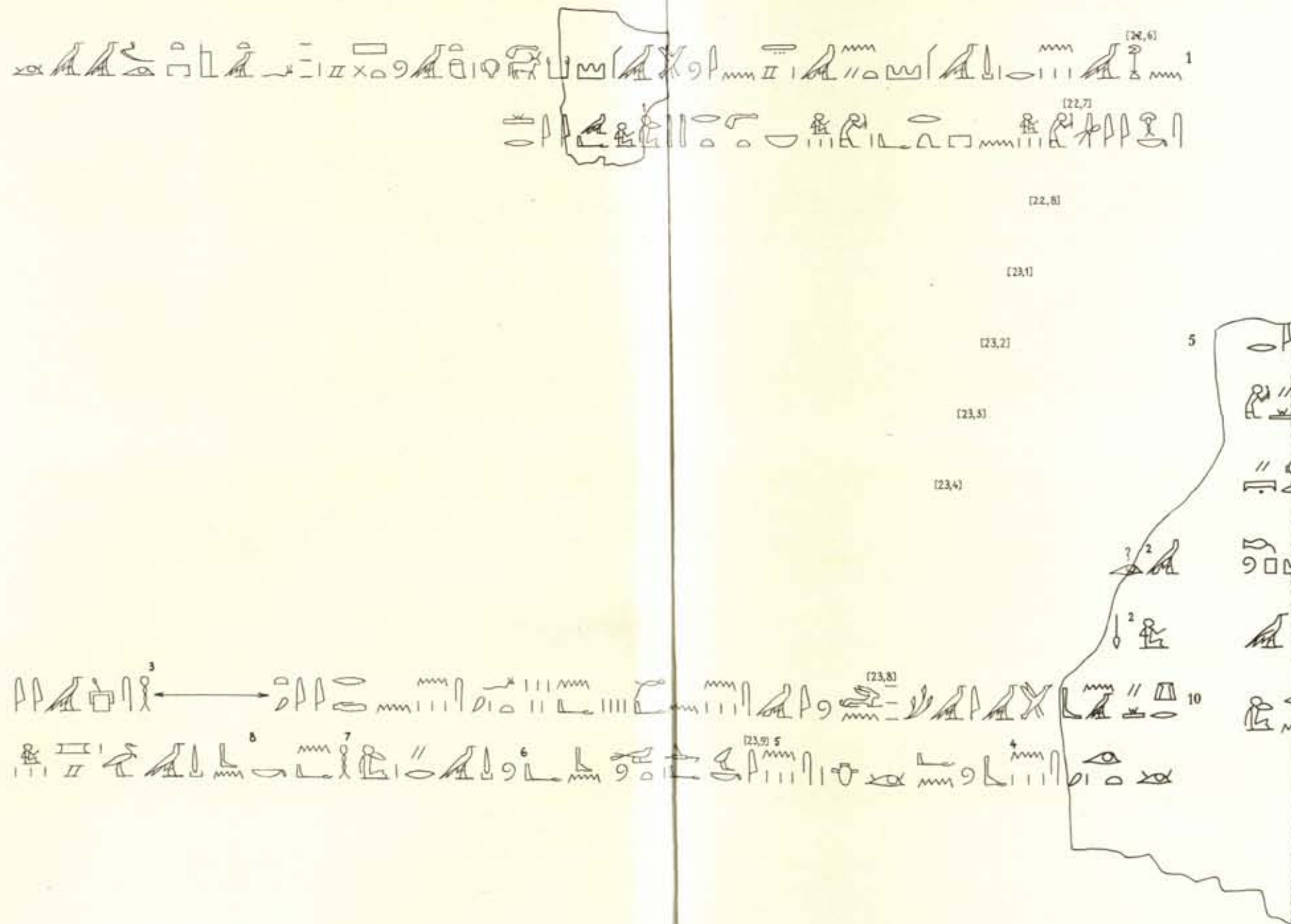
1. An. omits — of [21,2].
2. A trace that might belong to [21,3]; if so, read *mit.f* instead of *ti mit*.

[21,5]
[21,6]
[21,7]
[21,8]
[22,1]
[22,2]
[22,3]
12
10
16 [22,4]
15
[22,5]
23
22
21
20
19
18
17
14
13
10

NOTES

1. An. here inserts [22,4].
2. [22,5] is absent from An.
3. An. [22,6].
4. An. here inserts [22,7], which need not be superfluous, cf. *JEA* 27, 87 (16).
5. An. [22,8] instead of [22,9].
6. An. omits [22,10].
7. An. [22,11].
- 8-9. An. [22,12].
10. An. [22,13].
11. An. [22,14].
12. An. [22,15] instead of [22,16].
13. An. [22,17].
- 14-15. For the clearly differing place-names in An. see the publication.
- 16-17. [22,18] is substituted for [22,19] of An.
18. An. inserts [22,20].
19. An. [22,21] or [22,22].
- 20-21. An. [22,23].
- 22-23. An. [22,24].












































































































































NOTES

1. The trace suits $\hat{\mathbb{D}}$, not $\hat{\mathbb{A}}$ as in An.

2. Not identifiable in An. I, 23, 5-7.

- 3-4. An. has , and omits .

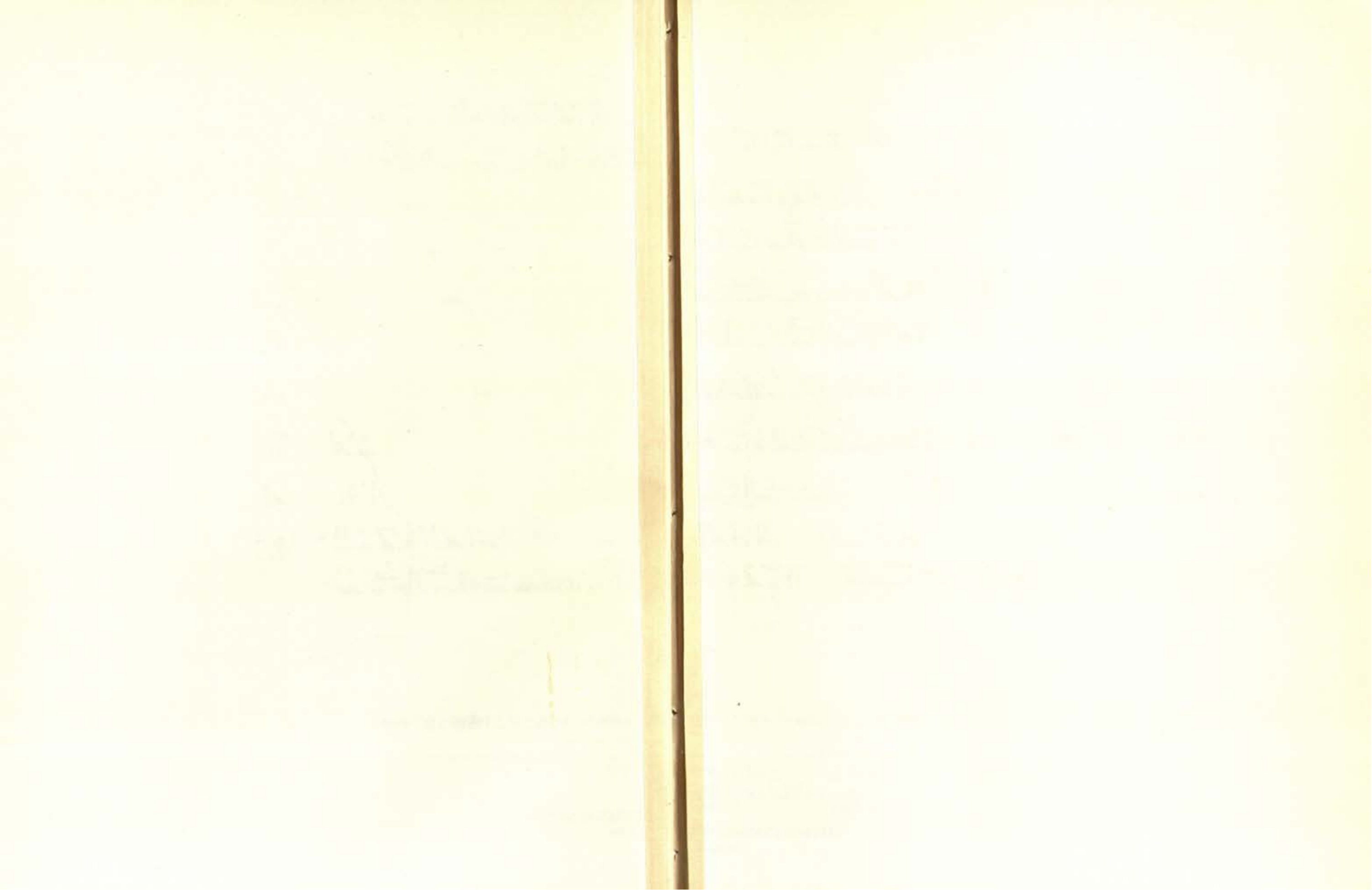
- 7-8. An.                                                                                                                                     

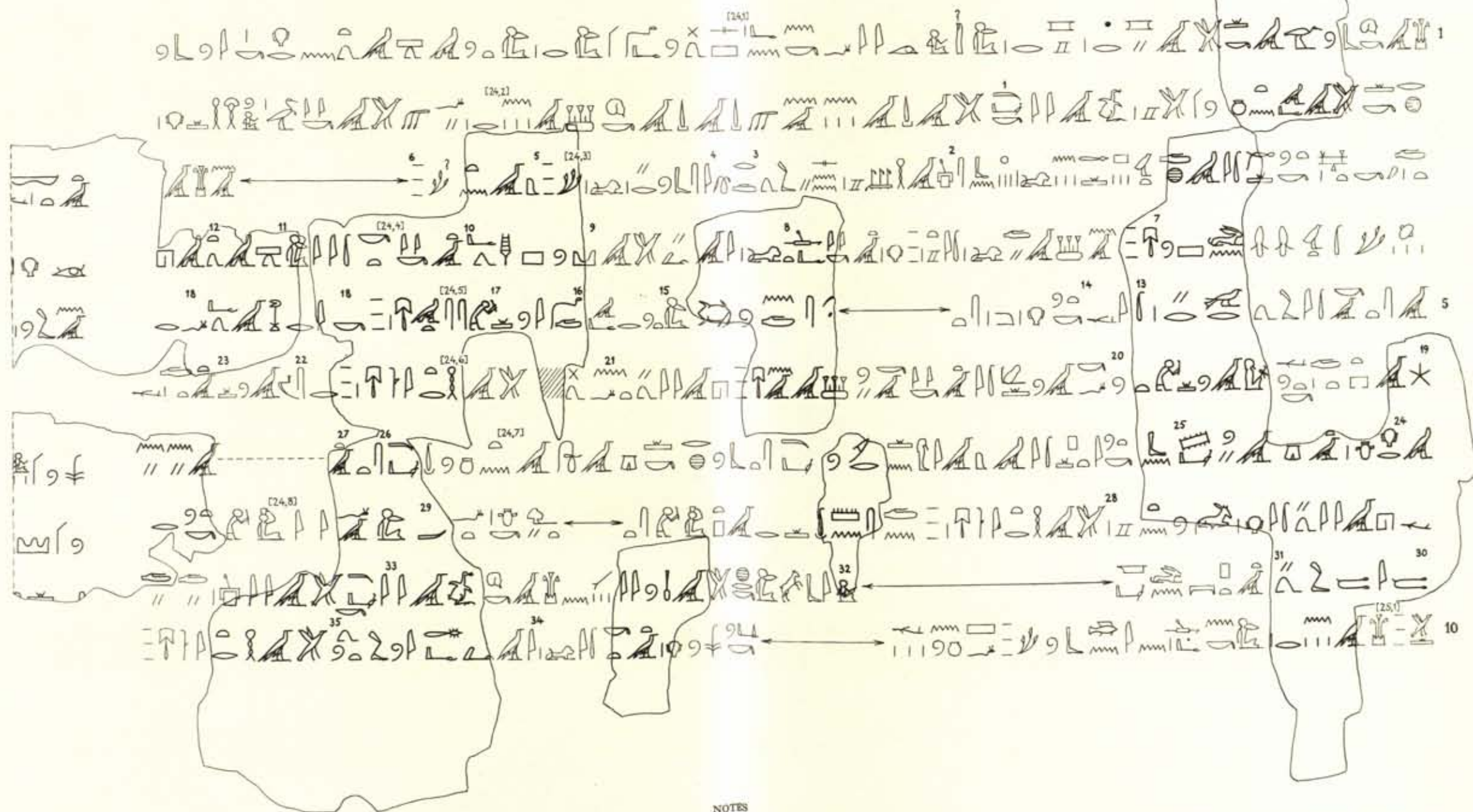
5. No room for $\frac{1}{2} \times \frac{1}{2} = \frac{1}{4}$, which
An. inserts after $\frac{1}{4}$.










6. An. lacks $\text{—}\epsilon\text{—}$, which is Sir Alan's conjecture based on 16, 2.








PAPYRUS FRAGMENTS IN THE TURIN MUSEUM

(= An. I, 22, 6-7 and 23, 7-8)
















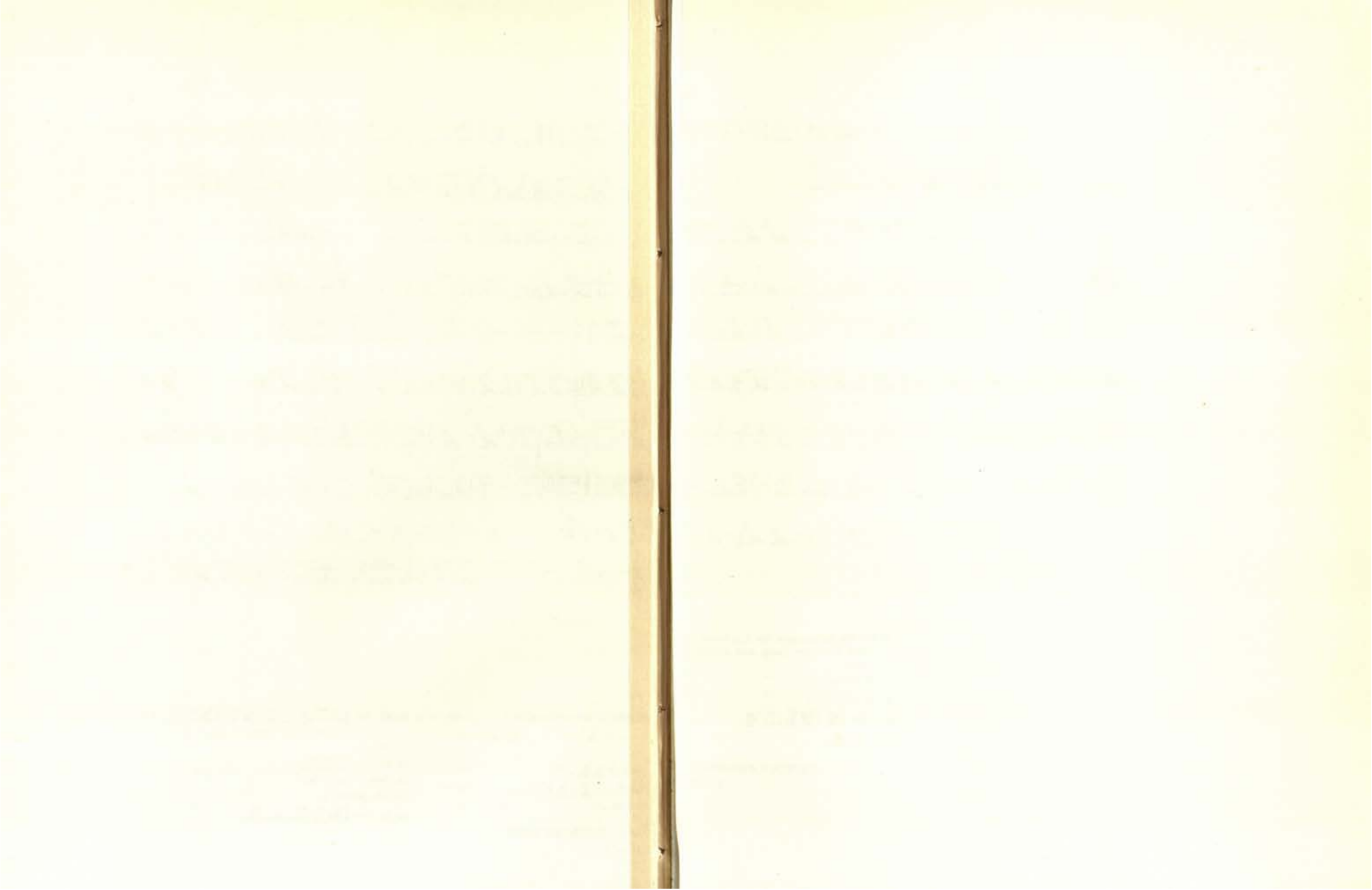
1. An. corruptly .
2. An. wrongly .
- 3-4. An. .
- 5-6. An. , omitting  before *hr*.
7. An.  instead of .
8. An.  instead of .

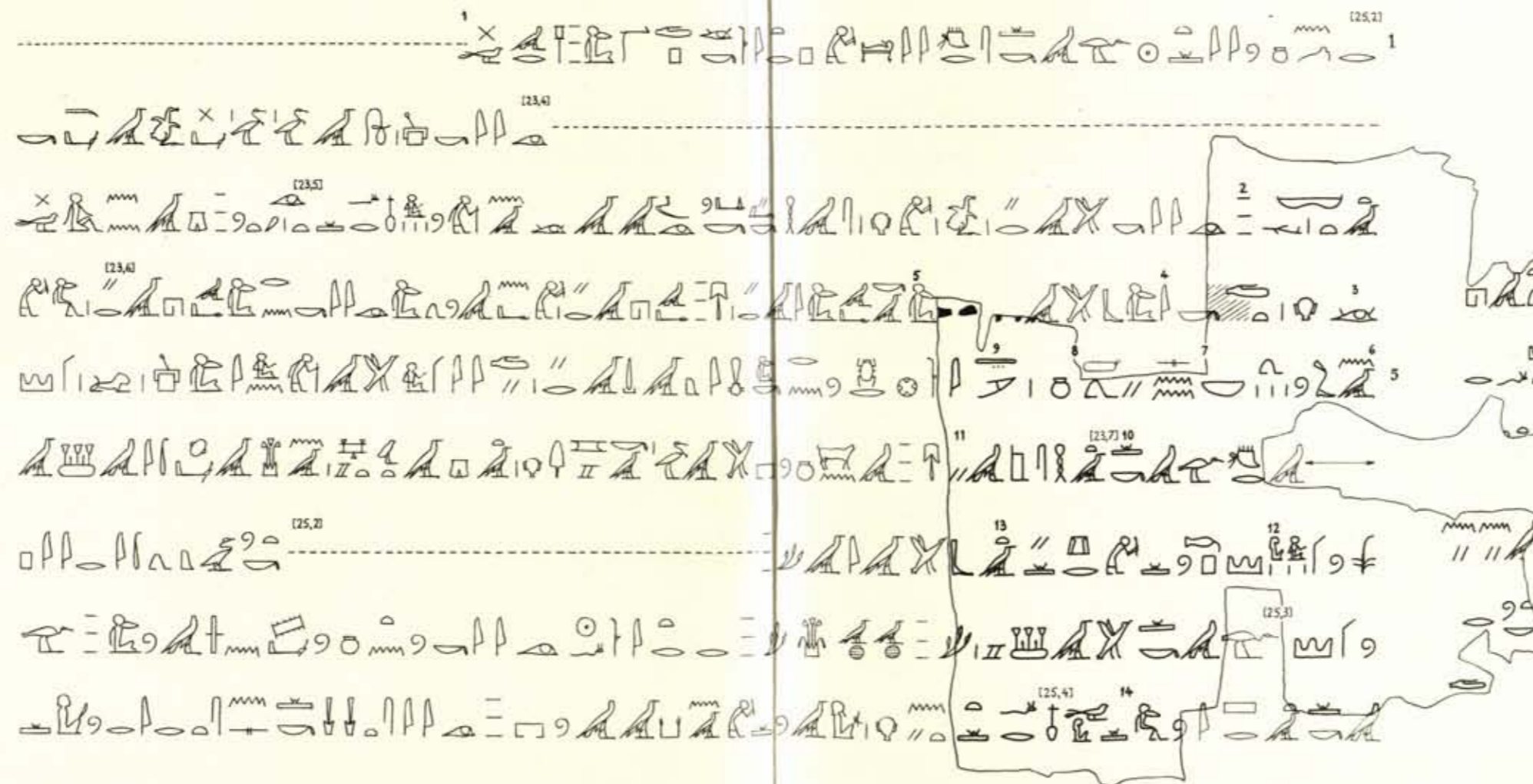
- 9-10. An. .
11. An. inserts .
12. An. inserts  under .
- 13-14. An. .
15. An. inserts .
- 16-17. An. .

NOTES








18. An. inserts  before the suffix.
- 19-20. An. .
21. An. .
- 22-23. An. .
- 24-25. An. .
26. An. inserts .










- 27-28. An. . For *hry-ti hr itwn* in our version cf. An. I, 26, 1.
29. An. .
- 30-31. An. .
32. Not in An. I, 24, 8.
33. An. omits .
- 34-35. An. .





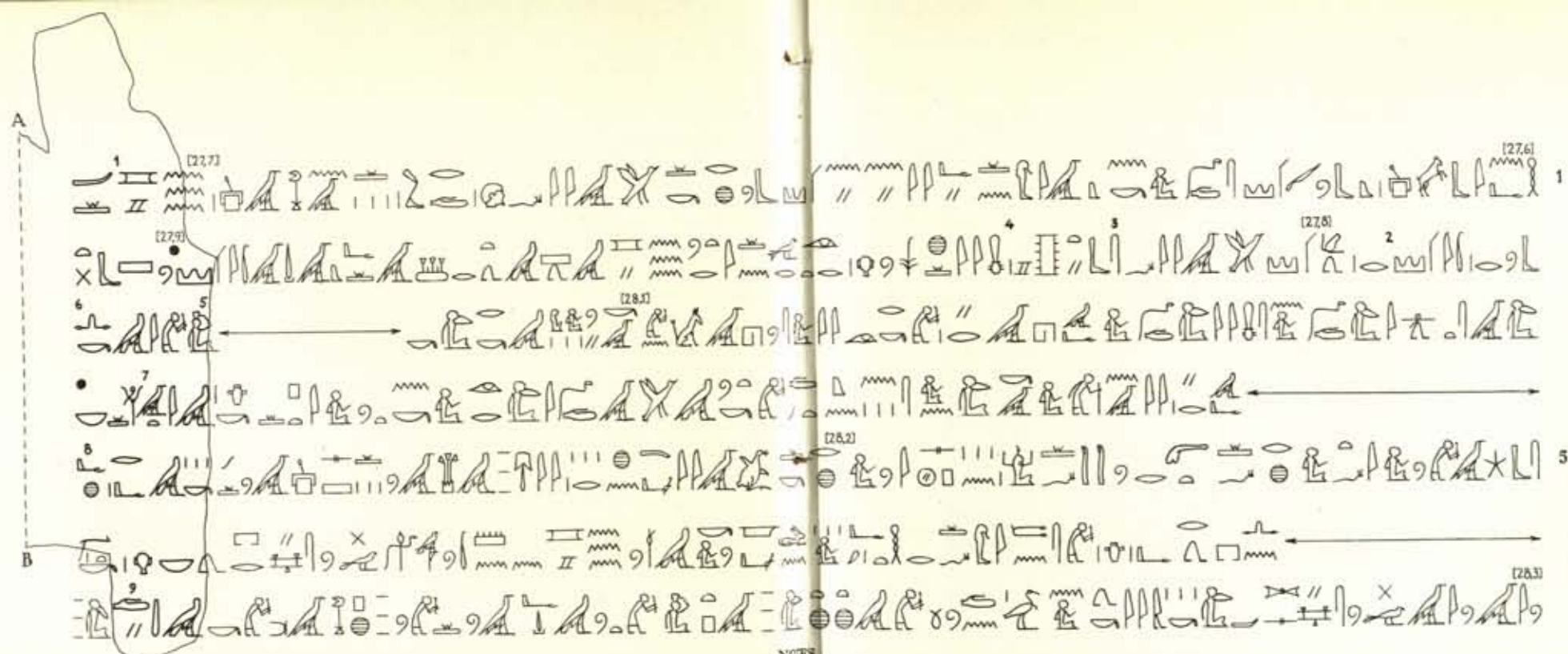
NOTES

1. For the continuation see l. 7 below. From about this point the Turin text inserted five or six lines.
2. An. omits .
3. An. omits .
- 4-5. An.    .
- 6-7. An. .

8. An. inserts .
9. An. .
10. An.  instead of .
11. An.  instead of .
12. An. omits .
13. An. .
14. An. omits .

PAPYRUS FRAGMENT IN THE TURIN MUSEUM


(= An. l. 23, 4-25, 4)





NOTES

1. An. inserts \int_{∞}^{∞} ; the r is probably superfluous.

2. For the passage omitted here see the publication (An. I, 27, 7).
- 3-4. An. $\left[\begin{smallmatrix} \infty \\ \infty \end{smallmatrix} \right]_{\text{II}}$, probably a corrupt reading.






- 5-6. Not in An. I, 28, 1.
7. An. 



8.  is not in An. I, 28, 2.
9. An. .



NOTES

1. \ominus is perhaps borrowed from $\textcircled{\ominus}$. An. omits \ominus^e .
- 2-3. An. has *dd'i*, and later *phr-i* and *sīm-i*; the Turin version must have written *tw-i hr dd*, &c.

4. An.  .
5. An. omits  and inserts  after the town-sign.
6. Cf. *Wb.* II, 481, 9 and Coptic $\epsilon\alpha\mu\theta$. An. has .

7. An. omits .
8. An. ends at about this point.
9. Tail of  under the *m*.

A FRAGMENTARY DUPLICATE OF PAPYRUS ANASTASI I IN THE TURIN MUSEUM

By RICARDO A. CAMINOS

It is entirely due to the kindness of Sir Alan Gardiner that I am able to publish here the remnants of a duplicate of the lengthy Ramesside composition which is preserved *in toto* in the British Museum papyrus no. 10247, a hieratic manuscript better known as Papyrus Anastasi I, the topic of which is a learned controversy in the guise of a letter from the erudite scribe Hōri to his friend and colleague Amenemopē. The hieroglyphic transcriptions of the papyrus fragments that appear on the plates were made by Sir Alan directly from the hieratic originals preserved in the Museo Egizio at Turin. He also arranged and placed the papyrus fragments, and supplied me with elaborate drafts from which my final drawings were executed. Furthermore, most of the notes on the plates were written by him. It is a pleasure to acknowledge my deep gratitude to Sir Alan for having entrusted me with the publication of his material.

There follows a conspectus of the contents of the accompanying plates.


Plate II: Two papyrus fragments previously published by Farina, *Riv. Studi Orientali*, 13, 318–20. They have preserved the better part of one page containing a duplicate of An. I, 8, 8–10, 7. These fragments and those mentioned below formed part of one and the same papyrus.

Plate III: Fragments from two consecutive pages. The small piece reproduced at the top of the plate holds the remnants of the last three lines of a page, the text being that of An. I, 21, 2–4. The six fragments in the lower portion of the plate must clearly have belonged to the next page of the same papyrus; the page contained a duplicate of An. I, 21, 4–22, 5.

Plate IV: Fragments appertaining to the page following that reconstructed on plate III, bottom. The page appears to have held a duplicate of An. I, 22, 5–23, 9.

Plate V: Fragments belonging to the page following that of which only the two pieces on plate IV remain. Duplicate of An. I, 23, 9–25, 1. Published by Farina, *op. cit.* 320–1.

Plate VI: A fragment of the next page of the papyrus bearing parts of the text of An. I, 23, 4–25, 4. Published by Farina, *op. cit.* 320 (II).

Plate VII: Fragments from two consecutive pages. The fragment given at the top of the plate is the upper left-hand end of a page; it holds words or parts of words belonging to An. I, 27, 7–28, 3. The five fragments reproduced underneath obviously belonged to the next page of the papyrus; they duplicate the last two lines of An. I (28, 7–8) with an addition or continuation which is absent from the British Museum version. The text of these five fragments has been published by Farina, *op. cit.*, p. 322. On the verso of the larger fragment are the remains of four lines of an administrative cursive text, the top line reading 

Great care has been taken to show on the plates, in outline, the size and shape of the Turin papyrus fragments as accurately as possible. I have drawn the extant textual matter with a heavy line, and all restorations with a light line. The superlinear numbers in square brackets are the column- and line-numbers in P.Anastasi I. The restorations follow as closely as feasible the British Museum version of the text; deviations from it are pointed out in the footnotes. For the purpose of comparison the student may utilize Gardiner's transcription of the entire contents of P.Anastasi I in his *Egyptian Hieratic Texts. Series I: Literary Texts of the New Kingdom*, 1, 2 ff. The lengthy restorations on the plates have been made with a view to helping the reader exactly to locate the Turin fragments; the text of P.Anastasi I, which is the only complete version of Hōri's letter known hitherto, has been deemed sufficient for the purpose, hence no attempt has here been made to incorporate readings from other sources. For additional duplicates of our text the interested reader may be referred to the list in Van de Walle, *La Transmission des textes littéraires égyptiens*, 69 f., supplemented by Posener, *Rev. d'Ég.* 6, 43, n. 3; also Černý and Gardiner, *Hieratic Ostraca*, 1, 34, under *P.Anastasi I*.

AN ARCHAIC REPRESENTATION OF ḤATHŌR

By A. J. ARKELL

As regards my Brief Communication with the above title, published in this *Journal* (vol. 41, pp. 125-6), Mr. Martin Burgess of the Department of Egyptology at University College, London, has shown great skill in reconstructing the bowl under my guidance and with invaluable assistance with the reliefs from Miss Marjorie Howard of the Institute of Archaeology. The result is shown in plates VIII and IX, and the actual reconstruction is described by Mr. Burgess in the article which follows. The photographs of the reconstructed bowl largely speak for themselves, and emphasize the importance of the bowl for students of religion in the First Dynasty. I have only to add to my original communication that study of the bird's head has convinced me that it is not a pelican as originally suggested, but the Saddle Bill or Jabiru Stork, *Ephippiorhynchus senegalensis* (Shaw); see Cave and Macdonald, *Birds of the Sudan*, 62, a conspicuous black-and-white bird standing over 4 ft. high, with a 12-in. red bill encircled in the middle with a broad black band. It is still fairly common on rivers south of the Sobat and Baḥr el-Arab. It was frequently represented on reliefs of late predynastic-protodynastic date, for example on the Carnarvon Ivory (*JEA* 5, pls. 1 and 2) and the ivory comb published by Theodore M. Davis (*JEA* 5, pl. 33), and it seems likely that its conspicuous black-and-white colour led to its association with Ḥathōr. Certainly the relief of Ḥathōr's head on this bowl makes it clear that she was the goddess of the night sky, and the selection of black-and-white porphyry as the material for this striking piece of temple furniture must have been due to its being reminiscent of the starry sky. One wonders therefore whether the bowl itself may have been used for magico-religious ceremonies intended to discern the will of Ḥathōr by interpreting reflections of stars seen in the bowl when filled with water.

THE RECONSTRUCTION OF THE HATHÖR BOWL

By E. MARTIN BURGESS and A. J. ARKELL

A TOTAL of twenty fragments of black-and-white porphyry, six from the Ashmolean Museum at Oxford and fourteen from the Petrie Collection at University College, London, all originally from Hierakonpolis, were recognized to be parts of one large fluted bowl. The reconstruction of this bowl has now been carried out, but the evidence for the exact size and shape of the bowl proved so slender that it is necessary to publish now not only photographs of the reconstruction, but also a statement of the evidence and a description of the methods employed.

Porphyries vary very much in their appearance. Not only does the colour of the matrix vary—in this case it is almost black with a tinge of green—but the colour, shape, size, and distribution of the white phenocrysts vary so much from one porphyry to another that we had no doubt that the fragments from the Ashmolean and from the Petrie Collection were part of the same bowl. Proof came, however, when joins were found between Ashmolean and Petrie Collection fragments. Where it was possible, the fragments which joined were stuck together, but in one case the only evidence of a join was the fracture of a white phenocryst and the total area of contact was too small to stick.

When all possible joins had been glued together, there were fourteen unconnected pieces, most of them quite small. There were three fragments of the base ring (one from the Ashmolean), three fragments of rim (all from the Ashmolean), a small fragment of wall (Petrie Collection) with a portion of the rope decoration above the flutes, part of one end of one of the handles (Petrie Collection) with pieces of wall (Ashmolean and Petrie Collection) joining to it. There were also other small fragments of wall from both museums, which contributed no evidence but which had to be incorporated in the reconstruction.

A problem which arises in cases like this, where fragments of one object are possessed by two or more museums, is that either some fragments have to be given up or reproductions have to be made. In this case it seemed best to make castings of all the fragments and to use them in the reconstruction. This method has the added advantage that, when the reconstruction has been finished, the original fragments can still be examined separately.

It was essential that the castings should be exactly the same size and shape as the originals, with no trace of shrinkage or distortion. They were made of black plaster-of-paris in rubber latex moulds by a technique already published ('Casting Small Antiquities from Latex Moulds', E. Martin Burgess, *Rubber Developments*, vol. 10, no. 1, Spring 1957, pp. 20–22, issued by the Natural Rubber Development Board). The black castings were impregnated with polyvinyl acetate in a vacuum chamber (*Museums Journal*, 54, 125–7) and the white phenocrysts painted on their surfaces. The bowl was reconstructed in plaster-of-paris with the castings included in it and placed in their correct relative positions.



(1)



(2)

RECONSTRUCTED FIRST DYNASTY PORPHYRY BOWL FROM HIERAKONPOLIS



1. Mouth of bowl



2. Base of bowl



3. Detail of rim: Ḥathōr



4. Detail of rim

RECONSTRUCTED FIRST DYNASTY PORPHYRY BOWL FROM HIERAKONPOLIS

The fact that the bowl was fluted, though it increased the work, made reconstruction possible, for since the diameter of the bowl changes, the widths of the flutes change. The size of the flutes on any single floating fragment was a guide to its position. The first step was to find out how many flutes the original bowl possessed.

The bowl has two handles and each must have spanned the same number of flutes with half the remaining flutes between them on each side. There must, therefore, have been an even number of flutes. The base ring, of which three fragments remain, is a representation of a grass ring bound with palm-leaves. From the fragments it could be seen that there were pairs of bindings with a space between each pair. The flutes start above the base ring, and some were found in association with one of the base fragments. To every pair of bindings and their intervening space there are six flutes. As there must be a definite number of bindings and spaces, the total number of flutes must be divisible by six. One set of bindings and a space more or less would make a big difference to the diameter of the base ring, and it was found that twelve sets, as well as being a convenient number to divide a circle into, produced a diameter which fitted the fragments. Twelve sets of bindings and spaces would mean that the bowl originally had seventy-two flutes carved on it. When working with hand-made objects it is never safe to assume that one deduction, like the one above, gives a correct answer. There are variations even on an object made with such precision as this bowl. It was necessary therefore to check the number of flutes and arrive at a figure by another method.

The two largest wall fragments (pl. VIII, 2), one with part of a handle and the other nearer the base, were linked by only the most tenuous of joins, far too small for sticking. In order to fix them in the correct relationship one with the other, they were laid flutes downwards and supported underneath until they were in the correct position with the internal tool-marks passing in smooth curves from one fragment to the other. Liquid plaster was then poured on to the inside surface of each fragment and the two lots of wet plaster connected by iron bars. In this way, the originals could be brought into the correct positions and later on the castings could be held together in the same way. Having brought these two largest fragments together, it was possible to make a horizontal plaster impression of an outside section of the bowl twelve flutes wide. This impression, with its base ground flat so that the flutes were vertical and the flat surface on a plane parallel with the planes of the base and rim, was moved about on a series of drawn circles. The circle it fitted was $18\frac{1}{2}$ in. in diameter and any nine flutes made an angle of 45° with its centre. If nine flutes made 45° , then 360° would require seventy-two flutes.

It is hard to describe the methods employed so that the reader of the above two paragraphs can see the process step by step in his mind's eye without diagrams and photographs. All that is essential, however, is the fact that two methods were employed to calculate the number of flutes, and both gave a result of seventy-two.

The reconstruction of the bowl could then proceed, starting with the base (pl. IX, 2). The three base fragments had to be arranged round a circle of the correct size so that, when the base ring had been completed in plaster, the existing bindings and spaces would be so placed that there was room to carve the missing bindings and spaces

between them. A cushion-shaped disk of plaster was made of which the upper contour fitted the fragments inside the base ring. The diameter of this disk, $5\frac{7}{8}$ in., was that which would make the base ring the correct size to carry twelve sets of bindings and spaces. The top surface of the disk was divided into twelve equal segments by lines radiating from the centre. The three fragments were fixed to the disk so that the beginning of each set of bindings and spaces was opposite one of the radiating lines. The spaces between the fragments were filled with plaster, and the locating disk removed.

The two largest fragments of wall were fixed together by the method already described. The castings were stuck with water-soluble glue to the plaster impressions of the insides of the fragments. The missing pieces near the join were replaced with plaster and then the backing was soaked in water to soften the glue and the two castings were left fixed in the correct relative positions.

The lower end of the largest fragment of wall did not join the largest fragment of base (pl. VIII, 2, and pl. IX, 2). However, the thicknesses, the sizes of the flutes, and the curvature of the inside striations were so similar that the bottom of this wall fragment must have occupied a position at about the same height as the top of the base fragment. It was decided, therefore, to fix the wall fragment so that its lower end butted up to the upper end of this base fragment. It had to be supported so that the flutes on both pieces were in line with each other, so that the horizontal curve of the wall was part of a circle whose centre was over the centre of the base ring, and so that the contour line of the wall in profile ran smoothly into the profile of the base fragment as it stood out from the base ring. When this had been done, it was possible to measure the distance to the centre of the bowl from the point on the wall where the impression of curvature had been made. This distance, $9\frac{1}{4}$ in., agreed with the previous calculation of the total diameter at that particular height. In other words, the correct placing of the wall fragment had been confirmed. The wall fragment was fixed in position with more plaster, so that now an internal and external outline of the bowl had been established to the top of the handles.

This internal and external outline had to be copied all round the base unit, so that the shape of the bowl was uniform on all sides. A plasticine former, keyed on to a backing of plaster to give it rigidity, was made inside the upstanding fragment of side wall. It reached to the centre point on the floor of the bowl and to the level of the top of the handle. This former was then moved about the centre point through 180° and then through 90° each way and so on, plaster being applied outside it, until the walls of the bowl had been built up all round.

To fashion the outside surface a plaster templet, cast from the outline of the wall fragment, was moved round the outside, the surface being carved down to fit it. Before flutes are attempted, it is essential to have an accurate surface to work on. This surface was at the level of the crests of the flutes.

More plaster was now added above the level of the handles, and so shaped that the inside and outside curvature of the bowl walls continued smoothly upwards.

The handles presented a number of problems, the greatest being that there was no evidence for their original length. Here again the flutes came to our assistance. The

handles must have been a certain number of flutes long, and one too few or too many would make a great difference in the proportion of the handles to the bowl. Evidence suggests that this squat type of bowl evolves from a wide mouth, narrow rim, spherical shape, and short fat handles to a more squat form with wider rim, narrower mouth, and longer and thinner handles. In any case the proportions of the various parts do seem to agree: i.e. if the rim is wider and the mouth smaller in relation to the external diameter, then the handles are longer and thinner.

In the University Museum in Manchester there is a large bowl of black-and-white porphyry which was also found at Hierakonpolis (Quibell and Petrie, *Hierakonpolis*, 1, pl. 38). Measurements were made of this bowl and of another which is in the Petrie Collection, University College, London (Petrie, *Prehistoric Egypt*, pl. 36, 64) and the proportions were used to help in deciding how long the handles on our reconstruction should be in relation to the external diameter and the mouth and rim diameters. The proportions gave us lengths for the handles of the Ḥathōr bowl of 5.02 in. to 5.5 in. The other bowl in the Petrie Collection gave proportions which would make the Ḥathōr bowl handles anything from 7.06 in. long to 8.88 in. long. According to the proportions the Ḥathōr bowl appeared to be somewhere between the other two bowls in development.

A porphyry bowl of similar design with a base ring and fluted surface, found at Naqada, now in Cairo and published by J. de Morgan (*La Préhistoire orientale*, II, 195), has handles with nine flutes on them. It is usual to have an odd number of flutes or mouldings on any object as the eye is drawn to the central one and unity is obtained. Nine flutes would make the handles of the Ḥathōr bowl $6\frac{1}{2}$ in. long.

The length of the handles having been decided, it was possible to cast them in their proper places with the mould cored to produce the horizontal hole. The only remaining part of one of these holes tapers slightly towards the centre of the handle. The drilling of the hole would have been done from both ends and it was found that, if the hole was to emerge on the other side at the level of the outside contour of the bowl, the same size and on the crest of one of the flutes, the handles had to be nine flutes long.

The flutes were now lightly drawn out all over the bowl and the remaining fragments fitted in. The most important of these was the small fragment with the evidence of the rope decoration (pl. VIII, 1). It was hard to place this fragment, and even now the rope may not terminate the flutes at the correct height. Once again the fluted bowl in Cairo mentioned above was used as evidence, and its rope appears to be midway between the top of the handles and the underpart of the rim. This positioning appeared to agree with the general shape of the fragment in question and with the size of the flutes on it. The other fragment whose original position was at all certain was a fragment of wall with, at its top, traces of the bottom of one of the handles. Not only was its thickness wrong, but there was not really room to put it under the left-hand end of the existing handle. It was finally placed under the right-hand end of the opposite handle (pl. VIII, 2).

Meanwhile the rim was being reconstructed (pl. VIII, 1; pl. IX, 1). There is no linking fragment between the rim and the bowl, so the evidence for the size and shape of the rim and mouth was worked out separately. One of the three fragments of rim not only carried parts of the Ḥathōr head but gave part of the top surface of the rim, the mouth, the

inside wall as it sloped away from the mouth, and a small portion of worked surface underneath the rim on the outside of the bowl (pl. IX, 3). From this fragment could be seen the depth of the mouth and the thickness of porphyry between the mouth and the underneath of the rim. One of the other rim fragments, the one with the left horn of the Ḥathōr head, showed the width of the rim outside before it joined the bowl (pl. IX, 3). Added together these fragments gave us the total width of the rim, which was 3 in. There was enough outside edge of the rim for the fragments to be compared with drawn circles. The total diameter was found to be 13 in., giving a mouth diameter of 7 in.

Two concentric circles of 7-in. and 13-in. diameter were drawn out on paper which was stuck on glass. On the smaller circle and inside it a vertical plasticine wall was built to the height of the depth of the mouth. Outside the outer circle another vertical wall was built to a height suitable for the outside of the rim. The paper between the two walls was now cut away and the fragments of rim could be moved round on the glass. Strips of glass were fixed on top of the outer wall projecting inwards so that they just touched the outside fragment of rim as it was moved round. Strips of glass were also arranged to overhang the rim area from the inner wall at the angle at which the inside of the bowl sloped away from the mouth. The inner fragment of rim was placed with the area of mouth surface against the inner wall. Then the mould was filled with plaster at one pouring. The resultant cast required little carving and the top surface of the rim was quite flat, an accurate surface for drawing out and carving the relief.

We are much indebted to Miss Marjorie Howard, of the Institute of Archaeology, London University, for her reconstruction drawings of the Ḥathōr head and the Jabiru Stork (pl. IX, 3. 4). Of the Ḥathōr we had a fragment with the left horn, its star, and part of one point of the six-pointed star over the brow. We also had the vital fragment with part of the neck of the Ḥathōr, the right ear and star, and part of the eye with its strange V-shaped lower lid. The nose was missing, as were the lips and eyebrows. In the reconstruction, the parts for which we had no evidence were copied from the Ḥathōr heads on the slate palette of Narmer, also from Hierakonpolis and of about the same date. These heads have chins, but the fragment from which we were working showed that our Ḥathōr had no chin.

The outline of the Ḥathōr head was drawn on the plaster rim round the cast of the existing fragment. Then the area of the left horn was cut away and the cast of the second fragment fixed in position. The missing parts of the head were then redrawn.

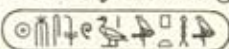
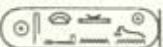


We did not know from what part of the rim the stork came; probably the rim was covered with carved relief, especially as porphyry is a hard stone and the less background there was to cut away the better. However, having no evidence for more relief, the stork was placed opposite the Ḥathōr head. First the cast of the fragment with the head and neck of the stork was fixed in the rim and then the reconstruction of the rest of the bird was drawn on the plaster. This drawing was based on the numerous representations of the Jabiru stork on ivories of the same period. The carving was now carried out, the rest of the rim being sunk to the level of the surrounds of the existing relief. When both the flutes and the rope decoration had been carved, the rim was positioned over the bowl and the two united with plaster.


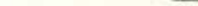
Finally, the plaster was hardened by painting it with polyvinyl acetate dissolved in toluene, the plaster surface was painted a dark greenish grey with water paint, and the whole varnished with a dilute solution of polyvinyl acetate.

ONLY ONE KING SIPTAH AND TWOSRE NOT
HIS WIFE

By SIR ALAN GARDINER

THE conclusions announced in the heading to the present article are in flat contradiction of what I stated, or at all events implied, in an earlier article published no more than four years ago. The evidence here to be adduced is, however, quite distinct from that which I previously used, and must, I think, first of all be considered on its own merits. This having been done, it will remain to discuss whether the contradiction cannot be somehow disposed of.

In 1912 there appeared, much delayed in the printing, an article by Daressy showing that on King Sethos II's death in his sixth year he was succeeded by a  Raḥmesse-Siptah with the prenomen  Sekha'enrē-setpenrē.¹ This unequivocal information was furnished by a limestone ostrakon found in the Bibān el-Molūk by Theodore Davis and better edited later by Černý in his catalogue of the hieratic ostraca in the Cairo Museum.² Just about the same time that Daressy wrote his paper the same prenomen and nomen came to light in a graffito discovered by Barsanti at Abu Simbel and published by Maspero in *Ann. Serv.* 10, 131 ff.³ Neither scholar was at the time aware of the discovery disclosed by the other, but their reaction was the same in both cases. In his excavation of the Serapeum of Memphis half a century earlier Mariette had found a small vase with the cartouches of Neferkarē Ramesses IX contained within a larger vase inscribed with the prenomen Sekha'enrē-meryamūn and the nomen Raḥmesse-Siptah⁴ and on the strength of this material proximity had argued that the Raḥmesse-Siptah in question must belong to the Twentieth Dynasty like the Neferkarē in whose company he was found. Both Daressy and Maspero realized the falsity of this deduction, and identified the Raḥmesse-Siptah of the Serapeum with the earlier king of the name with whom they were each separately concerned. The small difference of the epithet  at the Serapeum and the epithet  on the ostrakon and at Abu Simbel obviously could not stand in the way of these identifications.

This point being settled, there still remained the problem of the relation, if any, of Sekha'enrē-setpenrē Ra'messe-Siptah to a more often named King Siptah bearing the prenomen  Akhenrē-setpenrē and the nomen  Merenptah-Siptah.⁵ This is the matter now particularly interesting me, together with the date or

¹ *Rec. trav.* 34, 39 ff.

² No. 25515, see *Ostraca hiératiques* (CCG), pl. 9, with p. 12* of the text volume.

³ Also Porter and Moss, VII, 99, (11).

⁴ Porter and Moss, III, 207, under E'.

⁵ Gauthier, *Livre des rois*, III, 140 ff. On his coffin found in the tomb of Amenophis II the roughly written prenomen substitutes *mry-Tmn* for *stp-n-Rr*, the nomen not being given, see Elliot Smith, *The Royal Mummies*, pl. 60. There are many small variations in both cartouches.

a Horus-name as *Wr-phty* might indeed occur with more than one king,¹ but not *Mr[y]-Hꜥpy*. This argument acquires additional force from Ostr. Gardiner 10 recently published by Černý and myself in our *Hieratic Ostraca*, pl. 17, 4; here in a full titulary of Akhenrē-*[setpenrē]* Merenptah-Si[ptah] dated in his year 5 the Horus-name *Mry-Hꜥpy* receives the further adjunct *snh tꜣ [nb m kꜣf]* exactly as in the Abu Simbel graffito, where it is assigned to Raḥmesse-Siptah. This supplies a very strong ground for maintaining the identity of Raḥmesse-Siptah and Merenptah-Siptah, and taken in conjunction with the naming of Sety as Nubian viceroy, constitutes a wellnigh unanswerable case for that view; surely few scholars will accept the contention that because Sety was a very common name at this period there may have been more than one viceroy of the name. Von Beckerath,² who argued on much the same lines as I have done, made two very good points in observing (a) that while we possess separate tombs for Sethos II, Amenmesse,³ and Merenptah-Siptah, we have none for a separate Raḥmesse-Siptah, and (b) while of the few dated records of Merenptah-Siptah three belong to year 3 and one each to years 5 and 6,⁴ none belongs to year 1, the only year attested for Raḥmesse-Siptah. If we are asked to explain why King Siptah adopted a new form of name between years 1 and 3, we can only reply that we cannot do so, but that troubled events were evidently in progress and they must have been the cause. We can now add, however, that evidence is accumulating to indicate that royal names were by no means as stable and immutable as was formerly believed: Phiops I appears to have used the prenomen Nefersahor before finally giving the preference to Meryrē;⁵ if the arguments of Stock, von Beckerath, and myself are sound Menthotpe I changed his titulary not only once, but twice;⁶ and no one has, to my knowledge, contested the fact that Ramesses IV elected at the beginning of his reign to be known as Usimaḥrē-setpenamūn before substituting the element † *Hkꜣ*- for † *Ws[r]*-.⁷

Thus far I have added but little to the evidence adduced by others, but more remains to be said. Two Cairo ostraca,⁸ one of them that first published by Daressy, quote a vizier Praḥemhab who was in office in year 6 of Sethos II, and three graffiti in the Wādī Hammāmāt⁹ all associate him with the same king, whose cartouches he is seen worshipping in two of the cases. In the famous indictment for crime P.Salt 124 the accused Pnēb is said (rt. 1, 3) to have bribed Praḥemhab with six servants who had belonged to the accuser's father, and the words *wnw m tꜣty* 'who had been vizier' show that Praḥemhab no longer occupied that position. The more closely we study P.Salt 124, the more

¹ About this period it occurs also, so far as I can see, only with Gauthier, *Livre des rois*, III, 133, xiii; 136, xxviii; Ostr. Cairo 25560 vs.; so too doubtless at the end of P.Sallier I, *Late-Egyptian Miscellanies*, p. 88.

² *Tanis und Theben*, 72.

³ I find the name of this king given as Amenmose by a number of scholars who ought to know better, since the *s* is invariably written twice. On the analogy of Raḥmesse I here write Amenmesse.

⁴ Von Beckerath, op. cit. 71, wrongly states that Sety was still viceroy in year 6 and this error is repeated by Helck on p. 39 of the article quoted below, p. 17, n. 1. Some time before year 6 Sety had been replaced by a viceroy Hori who does not concern us here, see *JEA* 6, 49.

⁵ Möller in *ZÄS* 44, 129; also Anthes, *Felsinschriften von Hatnub*, p. 13 with pl. 4.

⁶ *Mitt. d. deutsch. archäolog. Inst., Kairo*, 14, 42 ff.

⁷ Gauthier, *Livre des rois*, III, 178.

⁸ Černý, op. cit. 25515, rt. 1, 3; vs. 4, 3; 25538, 2.

⁹ Couyat and Montet, nos. 46. 239. 246.

evident it becomes that Pnēb had taken advantage of the prevailing situation to purloin things that were to have been used at Sethos II's funeral (see rt. 1, 6-8) and had shown his disregard for the defunct king by quarrying stone at the latter's tomb for use at his own (rt. 2, 5). I gain the impression that the accuser Amennakhte was trying to get Pnēb removed from the office of 'chief workman' which he had obtained as a result of his bribe to Pra'emḥab and that the papyrus is likely to have been written very soon after Sethos II's death. If so, the picture can be combined with the long series of Cairo ostraca where Ra'messe-SiptaḤ first comes into view. Before the end of that king's first year a new vizier Ḥori¹ is found busying himself with the affairs of the necropolis, and he can hardly be other than the vizier Ḥori who is found together with the cartouches of Akhenrē-setpenrē Merenptah-SiptaḤ in a graffito copied by Petrie on the road leading southwards from the town of Aswān.² The same vizier is found associated with the same pair of cartouches on the highly interesting hieroglyphic ostrakon belonging to the Boston Museum edited by Černý in the present volume. These two pieces of evidence, combined with the ostrakon first published by Daressy, place the equation of Ra'messe-SiptaḤ and Merenptah-SiptaḤ beyond the shadow of a doubt, for no judicious scholar will wish to postulate two separate viziers of the name of Ḥori, each holding office under a separate king SiptaḤ. It is true that Weil, in his excellent but somewhat out-of-date work³ on the Ancient Egyptian viziers, classified the Ḥori of the Nineteenth Dynasty under six different heads, but both Černý and Helck believe that it is the same person who is referred to in every case, and that he lived on into the time of Ramesses III.

The main purpose of the article published by me in *JEA* 40, 40 ff. was to summarize the results obtained by my friend Caminos in an elaborate investigation of the tomb of Queen Twosre, no. 14 in the Bibān el-Molūk. Two representations of a king were found there accompanied by cartouches that had been erased and replaced by others, and I had asked Caminos to ascertain whether the names of Merenptah-SiptaḤ were the originals, as Lepsius following the example of Champollion had maintained, or whether the priority was to be given to Sethos II, as Ayrton had later asserted. Caminos devoted to this commission a care and a thoroughness such as I had never expected, making diagrams of every scene throughout the entire length of the tomb and adding comments wherever cartouches or royal titles occurred. This admirable work had, however, the disadvantage of being too extensive to be published in full, so that it fell to my lot to prepare a brief statement.⁴ Caminos's final verdict, supported by other competent scholars examining the tomb with him or independently, was decisive in favour of Lepsius: the cartouches of Sethos II had been superimposed upon those of SiptaḤ and not *vice versa*, suggesting that of the two kings Merenptah-SiptaḤ was the earlier and consequently to be distinguished from Ra'messe-SiptaḤ, known from the Cairo

¹ For references see Černý, *op. cit.*, Index, p. 115.

² Petrie, *A Season in Egypt*, pl. 10, no. 278. The cartouches of this graffito are given also in J. de Morgan, *Catalogue*, p. 27, no. 208, but without the name of Ḥori.

³ A. Weil, *Die Veziere des Pharaonenreiches*, p. 108, bottom.

⁴ It must be emphasized that the state of affairs there disclosed is considerably more complicated than is admitted in my summary. For complete knowledge of the facts scholars will need to consult Caminos's material in the Griffith Institute at Oxford.

ostrakon to have been Sethos II's successor. I confess I had hoped that Ayrton's view would prove correct, in which case the identity of Raḥmesse-Siptah and Merenptah-Siptah would have been further confirmed, as I had long believed on the strength of the evidence set forth in the first part of the present paper.

Ayrton's contention having proved to be mistaken, it was logical to draw the opposite conclusion, and to admit that Merenptah-Siptah reigned before Sethos II, a conclusion which entailed the further admission that Merenptah-Siptah and Raḥmesse-Siptah were separate kings. Having now, however, had occasion to study the situation afresh, I am convinced that my original opinion was correct, and that accordingly the undeniable superimposition of Sethos II's cartouches over those of Merenptah-Siptah does not carry with it the chronological implication which others beside myself have over-hastily supposed. It is, in fact, becoming more and more evident that the chronological deduction from superimposed cartouches is highly precarious, and must give way to other considerations whenever these are sufficiently strong. The long and tedious controversy in which Sethe was the protagonist ended in the general acceptance of the view that the insertion of the names of Tuthmosis I and II over that of Ḥashepsowe was due, not to those kings, but to Tuthmosis III.¹ We need to realize that the carving of one pair of cartouches over another may sometimes have been the work of a person or persons interested to support the pretensions of a Pharaoh earlier in date. This does not mean, of course, that the secondary cartouches were never due to a king actually posterior in point of time; in the tomb of Twosre herself the last usurper everywhere was Setnakhte, and Setnakhte is known to have reigned later than any of the three other royalties there mentioned; so too the substitution of the titulary of Merenptah-Siptah over that of Amenmesse on two stelae in the temple of Ḳurna certainly corresponds to the actual historical order.² However, in the case before us, as well as in that of a block discovered by Petrie at Memphis where precisely the same superimposition has been found,³ there are ample grounds for denying that Sethos II was alive at the time, as will be seen by considering some of the consequences which would have ensued if Merenptah-Siptah had preceded him as king.

In the scene on the right-hand wall of the Entrance Corridor Twosre is seen standing behind Merenptah-Siptah and described as King's Great Wife. Siptah would thus have been her husband, since *ex hypothesi* her marriage to Sethos II took place only later. Can we imagine that a proud Pharaoh of adult age would have suffered his spouse to equip herself with a great tomb in the sacred burial-ground from which queens had hitherto always been banned?⁴ And if Twosre was already in possession of such a tomb when she became the wife of Sethos II would she have condescended to wear a bracelet depicting her standing humbly to pour wine into the goblet of her seated husband?⁵ And

¹ See particularly Edgerton, *The Thutmosid Succession* and the summing-up in Vandier and Drioton, *L'Égypte*, 3rd ed., 383.

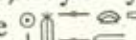

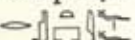
² See Caminos's article in Firchow, *Ägyptologische Studien*, 17 ff.

³ *Riqqeh and Memphis*, vi, pl. 57, 23, with p. 33.

⁴ It is true that Ḥashepsowe had a tomb in the Valley, but she had it in virtue of her claim to be King of Egypt, not a mere queen.

⁵ Th. Davis, *op. cit.*, pl. [11].

lastly, if a separate king Raḥmesse-Siptah had succeeded that husband upon the throne, would Twosre ever have found the opportunity, which she ultimately did find, of declaring herself King of Upper and Lower Egypt?

It is doubtless impossible, with the meagre data at our disposal, to present a historical picture accurate in all details, but at least some features of the situation stand forth clearly, so that a tentative reconstruction may be justifiable. This will be found to differ in some respects from that given by Helck in an article¹ with which I became acquainted only after half of the present paper was written, but at all events one must admire the skill and ingenuity with which he has stated his case, and the industry with which he has collected all the relevant material. This having been said, I find it better to go my own way. That Twosre was at some time Sethos II's principal wife is certain from the jewellery of which one item has been mentioned above. She may have been preceded in that position by a King's Great Wife Takha'e mentioned on a colossal group in the Cairo Museum where she appears to stand beside Sethos II.² However, notes which I owe to the kindness of Edgerton state that he found the name of Sethos II to be secondary on this statue, as well as on another of the same type which he saw in the temple of Karnak; this Takha'e was possibly a daughter of Ramesses II,³ and almost certainly the mother of the ephemeral Pharaoh Amenmesse,⁴ but she does not concern us here. To return to Twosre, it now seems certain that wherever she is designated King's Great Wife, the relationship intended was due to her marriage with Sethos II; a good reason for refusing to recognize her as the wife of Merenptah-Siptah has been given above. But if Siptah was not her husband, what can have induced her to depict him as her partner in her own tomb? The explanation may, I fancy, be found in the role played by the great minister of state Bay who likewise took the liberty of providing himself with a tomb in the Bibān el-Molūk.⁵ As Helck cleverly argues, Bay may have been of Syrian origin, since he is given at Aswān⁶ the additional name  Raḥmesse-Khaḥmentēru resembling other names given to foreigners who rose to high stations at the Court.⁷ In three distinct places⁸ he assumes the proud title of  'Great Chancellor of the entire land', and in each case he is shown in close proximity either to a figure of Merenptah-Siptah or to that king's cartouches. Of great interest is the attribute  applied to Bay in the West Silsila inscription and again in corrupt form in that from Aswān.⁹ This de Rougé had translated 'establishing the king on the seat of his father',¹⁰ a phrase curiously reminiscent of an epithet *smn wrw hr st-sn* 'establishing the great ones on their seats' given to the important Nubian official

¹ *Zur Geschichte der 19. und 20. Dynastie in ZDMG* 105, 27 ff.; see particularly 44 ff.

² Borchardt, *Statuen und Statuetten (CCG)* 4, no. 1198, with pl. 169.

³ Gauthier, *Livre des rois*, III, 112, no. 57.

⁴ *Ibid.* 129-30.

⁵ No. 13, see Porter and Moss, I, 18. The name is lost, but the owner's title suffices to prove his identity.

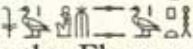
⁶ Leps. *Denkm.* III, 202c, reproduced with all evident mistakes in J. de Morgan, *Catalogue*, 28, 6. This is one of the stelae where the viceroy Sety is also depicted.

⁷ See further below, p. 21.

⁸ At West Silsila, Porter and Moss, v. 211 (38); Aswān, see above p. 13, n. 1; and at Amada, Gauthier, *Le Temple d'Amada*, pl. 21, and p. 108, where the legend is copied badly, corrected later in *Ann. Serv.* 24, 9.

⁹ See the last note for references.

¹⁰ E. de Rougé, *Œuvres diverses*, III, 291. Reisner, loc. cit. 49, agreed with the view taken here.

Hori son of Kama in the same reign.¹ It is incomprehensible to me why Breasted,² later approved of by Maspero,³ should have preferred the rendering 'whom the king established in the seat of his father', a weak and almost meaningless alternative, certainly more open to grammatical objection than de Rougé's version. Bay's claim to have been a 'king-maker' is both striking and probable. His title and name are preceded in the Aswān graffito by the words *rwī grg, dī mrt* 'banishing falsehood and granting truth', while in a damaged inscription at Thebes,⁴ Bay addressing a hymn of praise to Merenptah-Siptah says *dī-n-i irt-i hr-k w·tw* 'I placed my eye upon thee alone', phrases which may or may not refer to Bay's instrumentality in raising Siptah to the kingship. It must be observed that de Rougé's interpretation implies that Siptah was the son of a king, who can only have been Sethos II, but it is doubtful whether he was the  'king's son Ra'messe-Siptah' who owned a Book of the Dead now in the Florence museum;⁵ note that Ramesses II had a son of the name of Siptah, but without the preceding adjunct Ra'messe.⁶ The small temple of Sethos II at Karnak⁷ was dedicated as a reward to 'the hereditary prince (*iri-prt*) and eldest son of the King Seti-merenptah', but of this son nothing more is heard. The mother of our King Siptah is unknown, but we may perhaps guess that she was a Syrian concubine. It looks as though at the time of his accession he was a mere boy unable to assert his own rights; if the mummy found in the tomb of Amenophis II in a coffin roughly inscribed with his name is really his,⁸ he was at death still only a young man,⁹ and he is known to have reigned more than five years.¹⁰ It is thus easy to conjecture that his Pharaonic status was the result of an agreement between Bay and Twosre, an arrangement to the advantage of all three. Twosre may have needed to be conciliated as the widow of Sethos II, and perhaps also as the rightful heiress, though in her tomb there is only one example of the title *rpwt* and that a somewhat doubtful one.¹¹ The scenes in her tomb where Siptah is depicted might well have dated from a time when she did not as yet venture to proclaim herself king. I picture her to myself as occupying much the same position towards the young king as Hšhepsowe had occupied towards Tuthmosis III, a sort of guardian during his minority. On the left-hand wall of her tomb there is a representation where at first sight she does not appear to be present; Siptah is shown offering Truth to Isis, who is described as 'Isis the great, the god's mother' and says, 'I give thee the duration of Rē, and the years . . .';¹² I cannot help asking myself whether Isis here does not symbolize Twosre in the act of bestowing the kingship on her step-son Siptah. The replacements

¹ Randall-MacIver and Woolley, *Buhen*, 38.

² Ancient Records, III, 274, 279.

³ Th. Davis, *op. cit.*, p. xix.

⁴ Naville, *The XIth Dynasty Temple at Deir el-Bahari*, II, pl. 10 K.

⁵ Naville, *Das ägyptische Tottenbuch*, Einleitung, 85.

⁶ Gauthier, *op. cit.* III, 100, no. 43.

⁷ Chevrier, *Le Temple reposoir de Sêti II à Karnak*, 39, 45, 46, 56; depicted behind Sethos II in pl. 7.

⁸ Daressy, *Cercueils des cachettes royales* (CCG), p. 218 and pl. 61; Elliot Smith, *The Royal Mummies*, pl. 60; the attribution receives some support from the fragment of a pottery vase with his name found in the same tomb, Daressy, *Fouilles de la Vallée des Rois* (CCG), no. 24880, p. 216.

⁹ Elliot Smith, *op. cit.* 72.

¹⁰ See the Wādi Ḥalfa graffito quoted above, p. 14, n. 4.

¹¹ Leps. *Denkm.* III, 201, b; the actual figure of Truth has there been omitted.

¹² *JEA* 40, 42.

of the name of Sethos II throughout the tomb might have been effected when she no longer wished her association with Siptah to be remembered, but was unable to suppress the fact of her marriage to his predecessor. Later days refused to regard either Siptah or Twosre as legitimate Pharaohs; in a procession of royal statues at Medinet Habu the immediate successor of Sethos II is Setnakht.¹

It would have been difficult to depict the close association of Bay, Twosre and Siptah more convincingly than is done in two complementary scenes on opposite sides of the entrance to the vestibule in the Nubian temple of Amada.² The name of the dedicatory is given in an identically worded column to right and left; this Piay, though not given that title elsewhere, was a 'captain of troops' (*hry pdt*) known to have visited Kush to receive tribute in the third year of Merenptah-Siptah.³ The Amada scenes must be later than year 1, since the form of Siptah's nomen is no longer Ra'messe-Siptah. To the left Bay is shown squatting in an attitude of adoration before the cartouches of Merenptah-Siptah; opposite, on the right, the 'god's wife, Great King's (Wife), mistress of the Two Lands, Twosre-loved-of-Mut' jingles her sistra in the direction of the royal cartouches. The relationship of the same three personages seems reflected in each possessing a tomb in the Bibān el-Molūk, and it is not impossible that all three tombs were started about the same time; that of Twosre was begun in year 2, doubtless of Siptah, as we learn from the Cairo ostrakon J. 72452, from the beginning of which, through the kindness of Černý, I was able to quote a translation in my previous article.⁴ Černý has now shown me another ostrakon (Cairo J. 72451) dated in a year 3, fourth month of Inundation, day 20, which records the amount of work done on that day in the tomb of the Chancellor Bay; there is a slight doubt about the reading of the title and name here, but it would be difficult to find a plausible alternative. The lack of animosity between Twosre on the one hand and Bay and Siptah on the other seems reflected in the presence of Siptah in the queen's tomb, but the good feeling was hardly reciprocal. In Siptah's own tomb the queen is never mentioned; it remains to be explained why, as I have learnt once again from Edgerton's notes, the king's cartouche was on the inner walls, with one solitary oversight, everywhere erased only to be restored later in crude paint. In the foundation deposits of the funerary temple of Siptah discovered by Petrie to the north of the Ramesseum Bay is constantly mentioned,⁵ but Twosre referred to only by a solitary stray scarab with her name written as Twosre-setpenmūt⁶ almost as in the Amada relief above described. Twosre's own funerary temple, situated to the south of the Ramesseum, must date from later, when the connexion with Siptah had broken down or ended with his death, for the foundation deposits all testify to her claim to be king.⁷ She had now adopted a second cartouche Sitrē-meramūn whilst retaining her old name in the form Twosre-setpetenmūt. At Thebes the title 'King of Upper

¹ *Festival Scenes of Ramses III* (Chicago, Medinet Habu, IV), pl. 207; cf. too pl. 203.

² For the reference, see above, p. 17, n. 8.

³ Randall-MacIver and Woolley, op. cit., pl. 12, 7 with p. 26; pl. 16, with p. 39; also p. 43.

⁴ *JEA* 40, 43, n. 3.

⁵ Petrie, *Six Temples at Thebes*, pl. 17, no. 12; pl. 18, nos. 10, 11, 12, 13.

⁶ Op. cit., pl. 16, no. 7, see p. 15.

⁷ Op. cit., pls. 16: 17, 2, the latter a sandstone block showing both cartouches preceded by *nb tꜣwy* and *nb hrw* respectively.

and Lower Egypt' prefixed to the new nomen Sitrē-mer[amūn] is found only once on a winejar.¹ Most of the pieces of glaze found by Petrie at Serābīt el-Khādīm show only the name Sitrē-meramūn, but one piece combines with this the second cartouche Twosrē-setpetenmūt.² But by far the clearest and most indisputable testimony to Twosrē's kingship is seen in two limestone bricks said to come from Ḳantīr³ of which one is reproduced in the accompanying figure; the inscription, which reads, 'The Mansion of Millions of Years of the King of Upper and Lower Egypt Sitrē-meryamūn in the House of Amūn, the son (*sic*) of Rē Twosrē-setpetenmūt', mentions this female king's funerary temple at Thebes, which may also have been referred to on the stela of Bilgai, though with only a single cartouche and that erased.⁴ Helck,⁵ who agrees with me that the stela of Bilgai commemorated a chapel in the Delta built by Twosre, preferred to restore the name of Siptah in the erased cartouche, and though thinking that in this he was wrong, I accept his ingenious identification of the steward Pbes mentioned there with the steward Pbes named on a wine-jar found in the funerary temple of Siptah.⁶



I no longer see any objection to attributing to Twosre the dates of years 6 and 7 found in a sideroom of her tomb,⁷ nor the date in year 8 contained in Ostr. Cairo 25293 published by Daressy;⁸ but it must be admitted that there is here no certainty. I have always believed that the Thuōris⁹ whom Manetho places at the end of the dynasty, with a reign of seven years, gives in distorted form the name of Twosre, though there misrepresented as a male. This would be the third example in Egyptian history of a woman bringing a dynasty to a close. There is good reason for thinking that Twosre was buried in her own tomb, since the jewellery found by Theodore Davis in an uninscribed cache¹⁰ must have been part of her funerary equipment, perhaps secretly hidden in order to save it from the rapacity of Setnakhte.¹¹ Her mummy is lost, unless it be that of a woman

¹ Op. cit., pl. 19, 2.

² See the references in Gardiner, Peet and Černý, *The Inscriptions of Sinai*, pp. 185-6 to Petrie's *Researches in Sinai*.

³ Hayes, *Glazed Tiles from a Palace of Ramesses II at Ḳantīr*, pl. 1, with p. 7.

⁴ *ZAS* 50, pl. 4, l. 15; see my note *JEA* 40, 44, n. 2.

⁵ Loc cit. 49. It is improbable that the stela erased the name of more than one royal person, and his restoration in the two earlier cases is grammatically impossible; a title following a proper name would have to be preceded by the definite article.

⁶ Petrie, op. cit., pl. 19, nos. 6. 10. The proper name is badly written or copied, but Černý agrees about the reading.

⁷ *JEA* 40, 43.

⁸ Ibid., n. 3.

⁹ Perhaps there has been some contamination with the Thuēris named by Plutarch, *De Iside*, 19 as Typhon's concubine; obviously she was the hippopotamus goddess *Tt-wrt*.

¹⁰ Th. Davis, op. cit., pp. 30 ff.

¹¹ As suggested by Lefebvre, *Muséon*, 59, 217.

found lying in the coffin-lid of Setnakhte among the royal mummies discovered by Loret in the tomb of Amenophis II.¹

Of direct relevance to the subject of the present article is the question of the Syrian usurper whom a famous passage of the Great Harris Papyrus (75, 4-5) places at the close of the period of anarchy and misery ended by the accession of Setnakhte and the ushering in of a new era of prosperity and happiness. Von Beckerath emphasized the traditional, purely literary character of this retrospect, but realized that the 'Syrian Arsu' must have been a real person,² perhaps a foreigner designated as his successor by Siptah. Helck, while agreeing with the general standpoint of von Beckerath, ventured the daring suggestion that this foreigner was none other than Siptah himself. Helck's arguments are well worth careful consideration, and much that he has written on the subject is perfectly sound. That the period between the end of the Nineteenth Dynasty and the beginning of the Twentieth was very short is proved by the continuance in office under Setnakhte and even later of both Siptah's vizier Hori and the latter's namesake the son of Kama who was viceroy in Nubia. Instances are then quoted of Syrian butlers who rose to high office under Ramesside kings and were given new names, like the *Rmssw-mrytmmir* whose Semitic name was *Bnšn*—Helck's collection of references here is very valuable. Unfortunately, however, the conclusion which he draws overlooks another far more plausible possibility pointed out to me by Černý. May not the Syrian Arsu (𓂏𓂛𓂏𓂛𓂏𓂛𓂏𓂛) have been none other than the chancellor Bay who played so large a part in placing Siptah on the throne? As mentioned already above, Helck himself pointed out that Bay received an additional name Ra'messe-Kha'mentēru' which suggests his foreign origin. If my conjecture holds good that Siptah was at his accession a mere boy and a pawn in the hand of Bay, Černý's guess, though admittedly no more than such, gains enhanced likelihood. Helck did not mention the epithet twice given to Bay on which depends his claim to have been a 'king-maker'; if de Rougé's translation of this was sound, Siptah was a king's son, and even if his mother was of Syrian birth that would surely disqualify him from being the Arsu mentioned in the Harris Papyrus. But it is time to call a halt to these speculations.

I am acutely aware that to have produced two diametrically opposite theories within a single quinquennium and without much new accession of facts is not calculated to inspire confidence. Having, however, reverted to my original view I felt it my duty to argue out the problem afresh. Of course I accept everything that Caminos has had to teach me about Twosre's tomb, though now repudiating the conclusions which I based thereupon. At the same time I do not guarantee the story which I have here tried to tell. More evidence is required before anything like certainty can be elicited in this troublesome problem.

Postscript

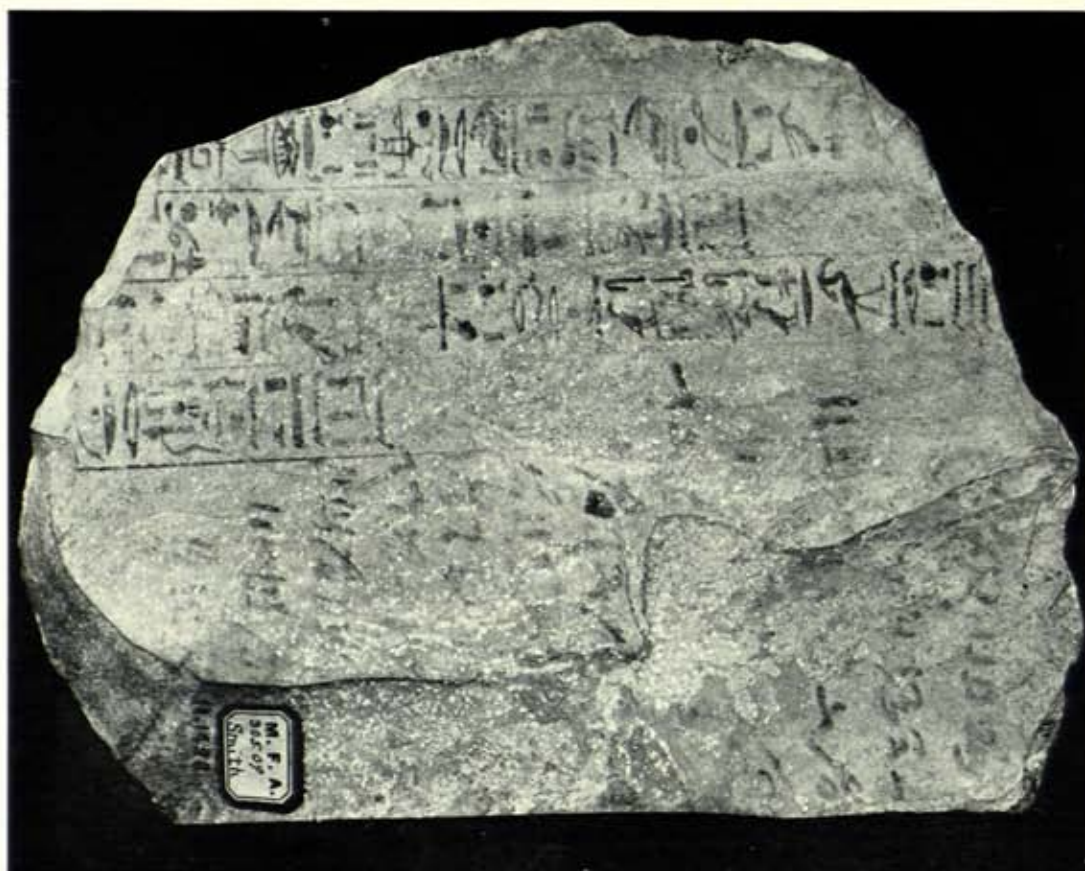
The present article was completed before I read L. Christophe's contribution to the

¹ Elliot Smith, *op. cit.* 81 ff.

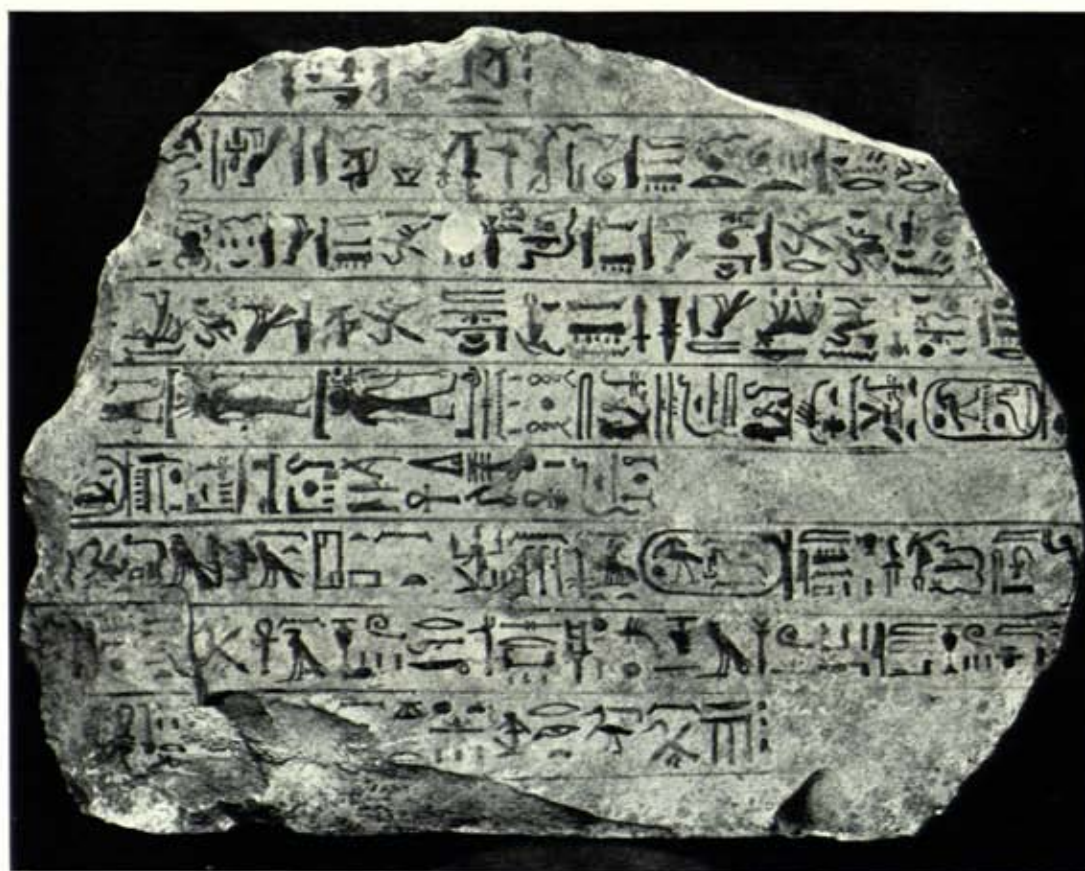
² *Tirso* is a not a Semitic name, but there was a necropolis-workman of the name of *Trit-stc*, e.g. Černý, *Ostraca hiératiques non littéraires de Deir el Médineh*, II, 164, II, 7. The strange Egyptian habit of giving altered or fictitious names to undesirable persons is well illustrated in the Lee and the Turin judicial papyri.

same subject in *Bibl. Or.* 14, no. 1, pp. 10-13. It will be found that his reconstruction differs fundamentally from mine. It was not until even later that my attention was drawn to von Beckerath's much more valuable article *Die Reihenfolge der letzten Könige der 19. Dynastie* in *ZDMG* 106, Heft 2 (1956), 241 ff. No useful purpose would be served, in my opinion, by a detailed discussion here of the points of agreement and of disagreement between us, and I will merely remark that the main difference between my view and that of my German colleagues seems to lie in the emphasis that I lay upon the youth of King Siptah and upon the role played by the chancellor Bay.¹ It is surely an otherwise unheard-of thing that an official, however important, should have intruded his name into his sovereign's foundation deposits.

¹ One small question addressed to von Beckerath: What evidence has he that Twosre was ever called a 'king's daughter', see his p. 242, under A(e)?



Verso



Recto

OSTRACON IN THE MUSEUM OF FINE ARTS, BOSTON

A HIEROGLYPHIC OSTRACON IN THE MUSEUM OF FINE ARTS AT BOSTON

By JAROSLAV ČERNÝ

THIS curious ostrakon in the Museum of Fine Arts at Boston (No. 11.1498) was copied by me as long ago as January 1955, and though Mr. Dows Dunham, who was then in charge of the collection, gave me permission to publish it at that time, it is likely that it would have remained unpublished for many years had I not rather light-heartedly promised Sir Alan Gardiner to write a note on it for the current number of this *Journal*, in which he himself refers to it (above, p. 15). I am therefore bound to honour my promise, albeit with some hesitation, since on the actual purpose of the ostrakon I cannot offer more than a guess and much of the inscriptions remains obscure to me. On the other hand, though the result of my efforts to interpret it is to me unsatisfactory, there is perhaps some justification in making known a document which is not clear in every respect, in the hope that others may succeed where I have failed.



The limestone flake measures 17 × 13 cm. and is inscribed on both sides. The photographs reproduced on pl. X (also kindly supplied by Dunham) speak for themselves, so that it is not necessary to describe the disposition of the texts on the two surfaces of the piece.




The side which I shall arbitrarily call the *recto* displays down the centre two vertical lines of hieroglyphs damaged at the top:


(1) [*A boon which the king gives to Amūn*], *Mut and Khons,*¹ *that they may give an eternity as a king of the Two Lands, everlastingness as a ruler and joy to the King of Upper and Lower Egypt Akhenrē-setpenrē, [Son of] Rē* (2) [*Ra-messe*]-*Siptah, beloved of Amen-Rē, King of the gods, Lord of heaven, and Ruler of the Ennead, given life, duration and dominion like Rē for ever and ever.*

To the left three lines of hieroglyphs run from left to right:

(1) [*Words spoken by*]² *Hori, Vizier and Steward of the Mansion of Millions of Years of the King of Upper and Lower Egypt Akhenrē-setpenrē in the estate of Amūn on the west of Wēse. He says in praise:* (2) . . .³ *Amen-Rē! Thine*⁴ *is life, favours are under thy authority, wealth, the duration of life, esteem and burial are by the command of thy ka. Give favour to the heart of the Vi[zier] (3) Hor[i], since he is valiant . . . under my possessions to be a servant of the Lord of the gods.*

¹ Figures of the three deities stand each on the hieroglyph  with their names written in front of them in small hieroglyphs. The top of the figure of Amūn is lost. *Di-sn* postulates the formula  at the top of the line.

² The tail of  is clear and the trace beneath suits . Restore .

³ Obscure traces resembling .

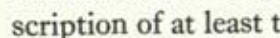

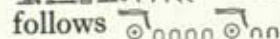
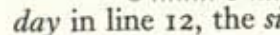
⁴ *Twt*, Erman *Neuäg. Gramm.*², § 109.

To the right of the two vertical lines is placed another hieroglyphic text, this time running from right to left:


(1) . . . of the herdsman of the vizier Ḥori, Pekharemwēse of Memphis, with (?) cattle entering into his possession to (?) Amen-Rē, (2) . . . his creations, Amūn being in Pnubs.¹ Thus said Amūn when making his appearance:² As for the vizier who shall move [this stela from] its [place],³ he shall not be satisfied with justice and shall not follow Amūn on any of his festivals. As for whosoever shall⁴ . . . (4) content of heart and following the Lord of the gods.

The verso (↓↑) contains in its right-hand half four vertical lines of hieroglyphs the beginnings of which are lost:

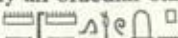
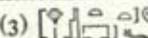
(1) . . . mayst thou wake up at (?) four and rise at daybreak (?),⁵ may thy limbs be clean so that thou mayst put on fine linen, (2) . . . [all] beautiful [things?], may thine heart get drunken and thy face brighten while thou art in joy⁶ every day, to the ka of (the rest of the line is left blank). (3) . . . good . . . are in the hands⁷ of Amūn. Things can be left to (?) him⁸ who knows how to keep safe whoever follows him and is useful to [his] partisan, (4) . . . every venerated one in (?) the light of the sun-disk of heaven, to the ka of (blank).

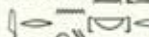
The left-hand half is occupied by twelve lines in hieratic written upside down in relation to the vertical lines of hieroglyphs. This hieratic is very faint, so that a transcription of at least the first two lines must be given here: (1)  (2) . Fourth month of summer, day 14. Thirty-ninth day of the Madjoy. Then follows  fortieth day, forty-first day, and so on till  sixty-sixth day in line 12, the sixty-third day and sixty-fifth day being covered by a modern label.

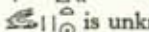
As Gardiner points out (p. 15 above), the main interest of the ostrakon lies of course in the association of the well-known vizier Ḥori with the king Raḥmesse-Siptah. It seems to have been set up as a substitute for an elaborate and therefore more expensive stela by the herdsman of the vizier Pekharemwēse. His name, unattested elsewhere, seems to mean 'He of (nA-) Khaḥemwēse', Khaḥemwēse being the famous high-priest of Memphis, and a son of Ramesses II. Pekharemwēse belonged to Memphis, as did also the vizier Ḥori.⁹ The provenance of the ostrakon, however, is more likely to be Thebes, where ostraca were commonly used for a similar purpose.¹⁰ The vizier Ḥori paid many


¹ The determinative  shows that P₁-nbs is a building or a locality, though probably not the town Pr-nbs, Προυνψ, in Nubia (Wb. II, 246, 1).

² Evidently an oracular statement made at a festival of Amūn.

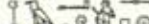

³ Restore  (3) , &c.

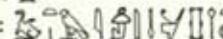
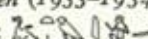
⁴  . . .

⁵  is unknown; is it a forerunner of wny, oꜣoem, 'light'?

⁶  . . .

⁷  .

⁸  with redundant .

⁹ Cf. the base of a stela Turin 9498:  (my own copy; quoted by Bruyère, *Rapport sur les fouilles de Deir el Médineh* (1933-1934), p. 56); also an unpublished hieratic ostrakon at Cairo from Mond's excavations at Kurnah: .

¹⁰ See, for example, Vandier d'Abbadie, *Catalogue des ostraca figurés*, nos. 2407, 2631, 2633, 2650, 2656; Brunner-Traut, *Die altägyptischen Scherbenbilder*, nos. 10, 14, 77, 78, 79, 82, 86, 87, 90.

visits to Thebes in the performance of his duties,¹ and was probably accompanied by some of his people from Memphis. The religious character of the ostracon did not protect it from being used profanely for a note concerning the length of the stay of the Madjoy-police.

It is perhaps worth mentioning that the forms of the hieroglyphic signs of the ostracon show a striking similarity to, if not identity with, those of a draughtsman responsible for the inscriptions in some of the tombs at Dēr el-Medīnah, about the end of the Nineteenth Dynasty. But the elaboration of this point requires much fuller demonstration than is possible or even desirable here.

¹ He is often mentioned in hieratic ostraca from the Valley of the Kings, cf. Černý, *Ostraca hiératiques* (CCG), nos. 25517δ, 1; 25536, 1; 25537, 1. 4; 25792, 6; 25794, 1. 2; 25831, 3.

THE TOMB OF A PRINCE RAMESSES IN THE VALLEY OF THE QUEENS (NO. 53)

By JEAN YOYOTTE

It is well known that four sons of Ramesses III had tombs prepared in the Valley of the Queens. At the end of the southern wādi are situated the tombs of Kha'emwēse (no. 44), of Sethikhopshef (no. 43), and of Pra'hiwenmaf (no. 42), while the tomb of Amen'ikhopshef (no. 55)¹ lies in the main wādi. As far as I am aware it has never been noticed that a fifth tomb in the site must also be assigned to a prince of the Twentieth Dynasty. This tomb, the entrance of which opens at a distance of 20 metres east of the entrance of the tomb of Amen'ikhopshef (no. 55), is numbered 53; its plan is given here, fig. 1.

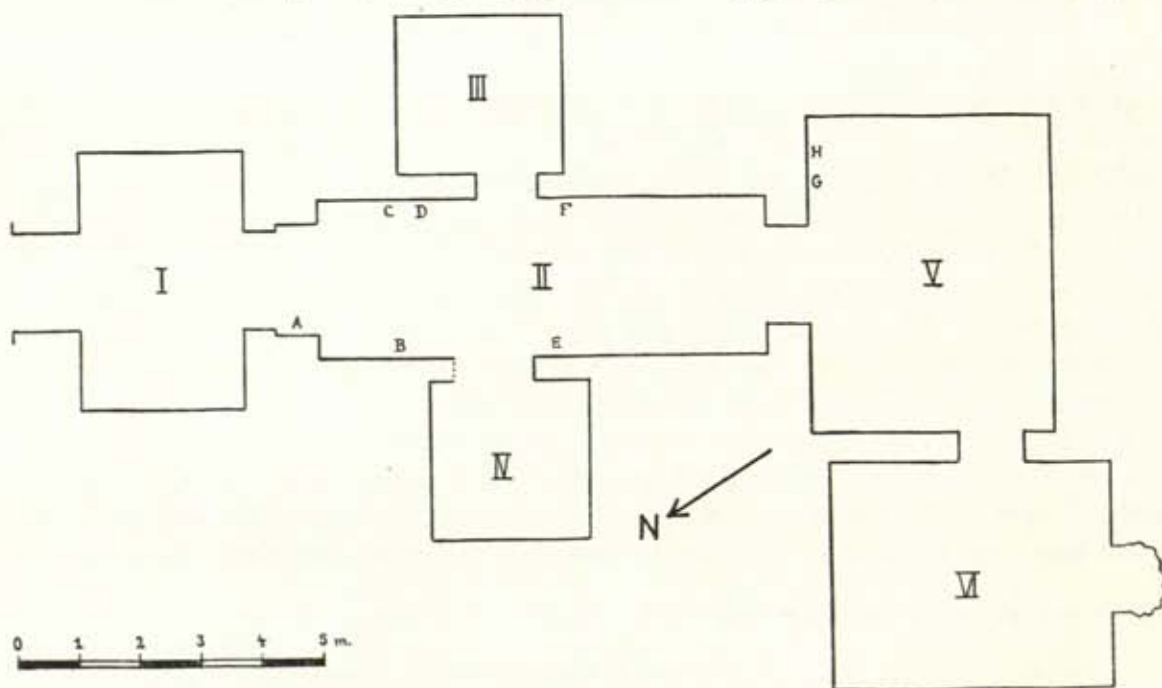


FIG. 1

It has never been cleared systematically. Its entrance passage and the vaulted ante-chamber (I) are blocked by such a heap of rubbish that the visitor is forced at first to proceed on all fours. The interior rooms (II-VI) are similarly obstructed by stones and flakes detached from the walls, though to a lesser height, so that to visit them is less of a hazard. All the rooms have suffered from a fire in the same way as those of the tombs of Sethikhopshef and Pra'hiwenmaf. Some walls are entirely blackened by soot, and of the thin layer of plaster which once covered them nothing still remains in place but

¹ Porter and Moss, *Top. Bibl.* 1, 40-41 and 44. See also Bruyère, *Bull. Inst. fr.* 25, 157-62.

shapeless patches baked and reddened by fire. The surface itself of the limestone has deteriorated almost everywhere. Lepsius had seen in the antechamber (I) a fragment of plaster casing containing the name and prenomen of Ramesses III written in the same orthography as in the four known tombs of this king's sons (Leps., *Denkm.*, Text III, 229, a); this fragment supplying the exact date of the tomb is now lost. Nevertheless, a few elements of decoration can still be identified in passage II and in room V.¹

In the passage (II) one can apparently discern two successive decorations superimposed. Originally the surface of the limestone was directly decorated with shallow painted reliefs. Subsequently these sculptures were re-dressed and covered by a coating of plaster which was again sculptured and painted. On both occasions the top of the walls of the passage was decorated with a frieze of *khakeru* exactly similar to those in tombs no. 44 and no. 55. Remains of the two superimposed friezes are clearly visible at various points of the passage (B; C-D; E; F); they were practically identical but did not exactly coincide.

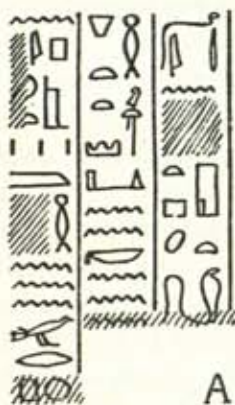


FIG. 2

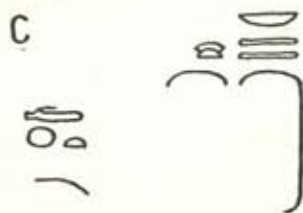


FIG. 3





















































































































































































































FIG. 4

The west jamb of the door of the passage preserves at A traces of the old *khakeru* frieze and of a design which is now indistinct. These designs are still covered by a large piece of plaster belonging to the second decoration. This fragment of casing, which bears a text in vertical columns, has lost all its colour, but the signs are still quite legible (fig. 2): 'Words spoken by [Nepht]hys, mistress of the West: I give thee water of Epet-sut(?) which is the great inundation (h'py wr) manifesting itself (hp[r]?). . . .' The signs show the fairly typical character shared by various tombs of the Valley of the Queens and especially by the four tombs of princes of the reign of Ramesses III (note in particular the form of 𓂏 with two loops only). The legend shows that the goddess Nephthys was here represented pouring water on the palms of her hands (*ny ny*) for the benefit of the deceased. This subject is fairly common in the tombs of Ramesside kings and princes; it is, however, striking to find a similar representation of 'Nephthys, mistress of the West' on the west jamb of the door of the passage—that is, in an exactly corresponding position—in the tomb of Amenḥikhopshef.²

¹ I have indicated in the present article all the elements of decoration which I was able to detect on three successive visits in April and May 1956. I noticed then that the poor remains were deteriorating more and more every day.

² Colin Campbell, *Two Theban Princes*, 74.

At D a few traces of painted signs survive on the surface of the limestone (fig. 4), showing that the passage (II) of tomb no. 53 was decorated with vignettes and texts taken from chapter 145 of the Book of the Dead, exactly as in the corresponding passages of the tombs of Amenḥikhopshef and of Khaṣemwēse;¹ the remains of phrases which can be restored ([*sbḥt* . . .] *nt wr[d]w[-ib]*; *rh·[kwy]*; [*rn*] *n [ntr] s; [tn]* belong undoubtedly to the opening common to the invocation of each of the 'porches' (*sbḥt*) enumerated in this chapter 145. The pharaoh whose head, titles, and protecting solar disk ([*Bḥ*]*dty*) are partly visible at point C (fig. 3) introduced the deceased to the porch. Now, the regular intervention of the king as mediator between the dead prince and the funerary gods or genii is precisely the well-known characteristic feature of the tombs of the sons of Ramesses III.² The remains of levelled-out texts reproduced in figs. 3 and 4 belonged to the earlier decoration, but a few pieces of plaster existing at the same spot seem to indicate that the same theme of porches had been adopted in the second decoration in plaster.³

The analogies between no. 53 and tombs nos. 42, 43, 44, and 55 would probably not suffice to prove that tomb no. 53 was that of a royal prince, if the only element of decoration of some importance still preserved in Room V had not miraculously transmitted the name of the deceased. At point G a large piece of plaster (fig. 5) still adheres to the wall. Its paint is relatively well preserved, and though the colours have deteriorated a little under the impact of the fire the same ornamental technique and the same painter's colour scheme are found here as in the tombs of the four royal sons already known.⁴ On the left can be read the end of a horizontal line of text which formed the legend of '[Anubis imy W]t, who presides in the god's booth'. From this it can be concluded that a recumbent Anubis, facing the door, occupied the upper register in the left-hand part of this wall.⁵ To the right of this Anubis one can see a standing person whose blue wig is partly preserved on the lower border of the piece of plaster; above this coiffure the end of the legend of this person still exists:                                                                                                                                                                                                                  

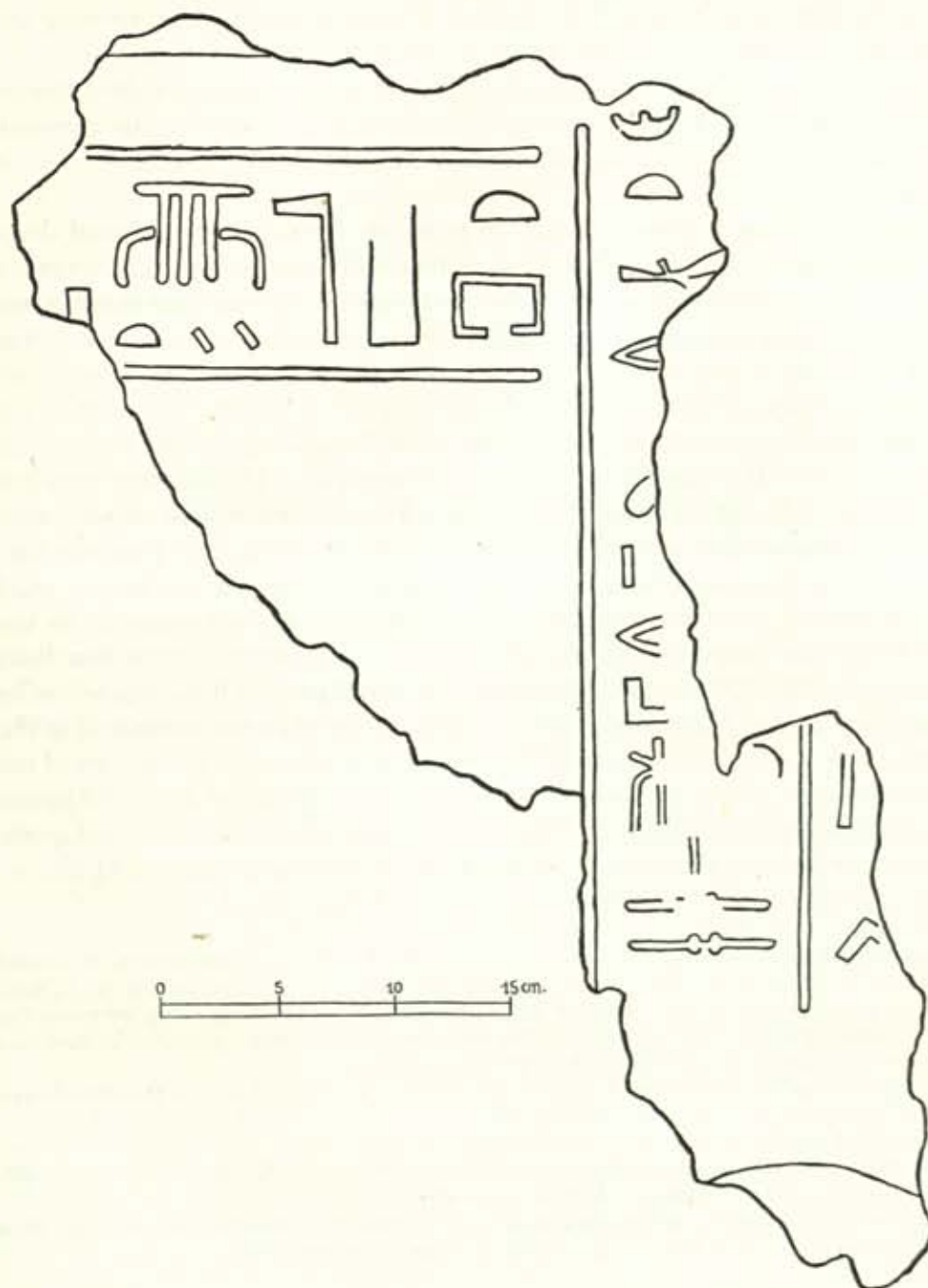


FIG. 5

preserves the title of *nb tswy* and the border of a vertical cartouche, proving that this Ramesses was preceded by the king. Judging by the size and the orientation of the signs preserved, these persons occupied the entire height of the wall and walked towards the interior end of the tomb. Like the scenes in the other princes' tombs, this representation therefore showed the ruler conducting his son and introducing him to the divinities of the netherworld.

The large fragment of plaster, which in principle belongs to the second decoration of the tomb—if, of course, room V ever received two successive decorations—is sufficient to prove that tomb no. 53 of the Valley of the Queens was made or re-made for the son of a king and of a great king's wife, a certain Ramesses. The situation and the plan of the tomb and likewise the epigraphy, the polychrome decoration, and the content of the scenes, which are comparable to or are identical with what is found in tombs nos. 42, 43, 44, and 55, suggest that this Ramesses, like the owners of these tombs, was a son of Ramesses III. The prince of tomb no. 53 is therefore very probably identical with the 'King's son Ramesses' who is known from at least two documents of the reign of Ramesses III, a lintel in Florence Museum¹ and a relief from the temple of Ramesses III at Karnak.² These two documents inform us that the prince was king's scribe and general (*mr-mšr*); the lintel proves that at a certain moment he was heir presumptive to the throne (*r-prt*).³ At first sight it is tempting to assume that Ramesses, king's son and general, is the same as the king's son and general Ramesses whose legends were inscribed later in the great temple of Medinet Habu on the south wall of the great court and in front of the first figure of the famous and enigmatic procession of princes.⁴ On the other hand, it may be that the Ramesses known from the lintel in Florence, the relief at Karnak, and tomb no. 53 is the prince who became later the Pharaoh Ramesses IV. On these latter points I leave the decision to others more conversant with the intricate problems presented by the genealogies of various kings named Ramesses.⁵

¹ Schiaparelli, *Museo archeologico di Firenze, Antichità egizie*, I, 332-3, no. 1602; Berend, *Principaux monuments égyptiens du Musée de Florence*, 101, no. 4019 (cf. Gauthier, *Livre des rois*, III, 176, n. 2). Schiaparelli indicates that the relief is of 'calcare coperto di una vernice gialla smaltata'. Berend says 'recouvert d'un émail grisâtre'. It would be worth while to verify whether the varnish of this stone—presumed to have come from the tomb of this Ramesses—is not in reality a layer of burnt plaster.

² *Reliefs and Inscriptions at Karnak, I, Ramses' III Temple*, pl. 18 A, col. 10; cf. Seele in *Ägyptologische Studien H. Grapow zum 70. Geburtstag gewidmet*, 309.




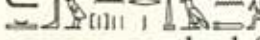
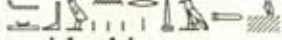

³ *Medinet Habu*, II, pl. 111, cols. 34-35; cf. Seele, *op. cit.* 299.



⁴ Leps., *Denkm.*, III, 214; statement of problems in Drioton-Vandier, *L'Égypte*, 3rd edn. (1952), 388-9, and Seele, *op. cit.* 300 ff.; Nims, *Bibliotheca Orientalis*, 14, 137-8.

⁵ J'adresse ici mes meilleurs remerciements au Prof. J. Černý—en compagnie de qui j'ai pu examiner le précieux fragment G—d'avoir bien voulu traduire en anglais le présent article.—J. Y.



QUEEN ĒSE OF THE TWENTIETH DYNASTY AND HER MOTHER

By JAROSLAV ČERNÝ

THE queen  () 'great King's-mother, mistress of the Two Lands Ēse', to whom belongs the tomb no. 51 in the 'Valley of Queens',¹ was a  'daughter' of , elsewhere in the tomb called  (in the passage, on the left) or . The parent with this strange name was held to be a man, and therefore the father of the queen, by Lepsius,² Brugsch,³ Sethe,⁴ Isidore Lévy,⁵ Gauthier,⁶ and Peet.⁷

Lepsius was led to this conclusion by the determinative of the name which he gives as ,⁸ but when I visited the tomb I saw no beard on the sign in my first example, and in the second the upper part of a sitting woman with  on the head could clearly be seen.⁹

Hbl̄nt or *Hbl̄t* was therefore a woman, the mother of Queen Ēse, and if so, this Queen Ēse must be identical with Ēse, the Queen of Ramesses III known from a statue of this king at Karnak,¹⁰ on which she is represented at his side. Lepsius¹¹ read her name

there as , but this can easily be emended¹² into  'Ēset, she

of¹³ (i.e. the daughter of) *Hmdrt*', and in view of the well-attested interchange of *m* and *b* in Egyptian, there can be no doubt that *Hbl̄(n)t* and *Hmdrt* are identical. The metathesis of *d* and *l* (or *r*) is no serious obstacle.

We now come to a third source where this name occurs, also, with a slight variation,

¹ For bibliography, see PM I, 41.

² Lepsius, *Denkm.*, Text, III, 234.

³ *Geschichte Ägyptens unter den Pharaonen*, pedigree opposite p. 456, and p. 618.



⁴ *Untersuchungen*, I, 62.

⁵ *Revue sémitique*, 8 (1900), 188-9.

⁶ *Le Livre des rois*, III, 174 (LXXXI, B). He registers Ēse twice, loc. cit., as wife of Ramesses III, and p. 201 (XXXIII) as mother (?) of Ramesses VI. To judge from his words on p. 174, he believed that these were two distinct persons though both named in the tomb no. 51; in reality only one Queen Ēse appears in this tomb.

⁷ *JEA* 11, 40.


⁸ Lepsius, *Denkm.*, Text, III, 234. His inscription is written from right to left and is now destroyed. It is identical in wording with my first example which, however, runs from left to right.

⁹ That Lepsius' wrong determinative is not a mistake of the editor of his notes is proved by the identical sign appearing in Lepsius, *Königsbuch der alten Ägypter*, I, no. 493. To suit better his entry Lepsius changed there—however incredible it sounds—the word  of the inscription into  'her father'!


¹⁰ PM II, 95.


¹¹ Lepsius, *Denkm.*, III, 207g.

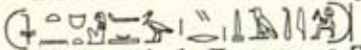

¹² The bad photograph in *Reliefs and Inscriptions at Karnak*, II, pl. 124, C, gives no help.


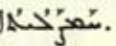
¹³ , from *t-nt* 'she of', Coptic τ-; see Erman, *Neuäg. Gramm.* § 127. Erman actually gives no examples of *t*, 'the daughter of', but see Spiegelberg, *ZÄS* 54, 107.

namely, two sandstone blocks seen by Lepsius at Dēr el-Bakhīt to the west of Thebes.¹ They show the top of a representation in which a naos was carried in procession on a bark surrounded by fan-bearing priests. The god in the shrine is 'Amen-Rē, Lord of the Thrones of the Two Lands'. In front of him stood the king, whose figure is lost, but who was 'Good god Usi[ma]rē . . .'. Above the representation are the remains of three lines of inscription which may be translated and understood as follows:²

- (1) ['Year . . ., month . . ., day . . ., under his majesty the King of Upper] and Lower Egypt, [Lord] of the Two Lands, [Usi]ma'rē, [Son of] Rē, [Lord] of appearances, Ramesses,³ the god, ruler of Ōn. On that day [the son of Amūn] of his (own) body [presented himself]⁴ before this god . . .
- (2) . . . [on] his beautiful [festival]⁵ of the Valley, while one was in the great forecourt of Amūn, to establish the name of⁶ the god's wife, pure of hands, of Amen-Rē, King of the gods, the King's daughter, Mistress of the Two Lands, the god's votaress Ēse. . . .
- (3) [in the presence of the King (?)] together with the King's mother  and the Overseer of the City, the Vizier Neḥy.⁷ And Amen-Rē, King of the gods, Mūt and Khons saluted her and foretold her good things till'




The title 'King's mother'  of this *Hmdrt* will have to be interpreted as 'King's mother-in-law', since she can hardly be the mother of Ramesses III and of his Queen Ēse as well.


Peet⁸ further tentatively identified *Hmdrt* of the blocks of Dēr el-Bakhīt with  whose tomb () was plundered towards the end of the Twentieth Dynasty.⁹ This tomb could hardly have been situated elsewhere than in the Valley of the Queens and is now either completely destroyed or one of the numerous anonymous tombs of the Valley.





The question of the identity of the names *Hbrdt*, *Hmdrt*, &c., has been settled by Dévaud¹⁰ who cleverly recognized that this is an Egyptian transcription of the Semitic name of meadow-saffron, Hebrew , Assyrian *habašillatu*, Syriac .




¹ Leps., *Denkm.*, III, 218a. b, better op. cit., Text, III, 101.


² For a slightly different translation, see Sander-Hansen, *Das Gottesweib des Amun*, 29.

³ Though Lepsius' copy suggests that  i.e. Ramesses III was later altered into , i.e. Ramesses VI, Ramesses III must have been the originator of the inscription.  . . . is given, though hatched, by Lepsius in the vertical line and the installation of the 'god's votaress Ēse' could only have taken place under Ramesses III and not under Ramesses VI in whose reign she was given a tomb, and therefore died and was buried.

⁴ The verb used was probably .

⁵ Read    .

⁶ Disregarding  as superfluous. Sander-Hansen translates 'to fix her name as' emending  into .

⁷  is fairly clear, but a vizier of this name is known only from the fragment of a statue found by Bruyère in the temple of Dēr el-Medīnah (Bruyère, *Rapport sur les fouilles de Deir el Médineh* (1935-1940), p. 109, fig. 186). Weil, *Veziere*, 112, note, interprets the signs as *ntyw* which gives no sense; the two titles of the vizier must be followed by a name.

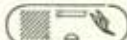
⁸ *JEA* 11, 40.

⁹ P.Brit.Mus. 10052, 1, 15-16; see Peet, *Tomb Robberies*, pl. 25, and Text, p. 139 and 143.




¹⁰ *Kémi*, 2, 7, n. 4, independently from Isidore Lévy, *Revue sémitique*, 8 (1900), 188-9. The latter explained well the name of the mother, but went, of course, astray as to the supposed name of the father.

The name of the plant¹ being feminine in Semitic, it is, of course, only suitable as a proper name for a woman, a further proof for *Hblḏt* being the mother, and not the father, of Queen Ēse.

Ēse was given her tomb as a 'favour of the King' Ramesses VI² and is there called the 'great King's mother' and 'Mistress of the Two Lands'; she was clearly the mother of Ramesses VI who died during his short reign. Ramesses VI was therefore a son of Ramesses III and not of a Prince Ramesses, otherwise unknown, as Sethe³ supposed. Ēse's mother Ḥabaḏilat, called 'King's mother' at Dēr el-Bakhīt and 'King's wife' in P.Brit.Mus. 10052, must have been the wife of one of Ramesses III's predecessors, though manifestly of foreign origin. She cannot have been merely the wife of an Asiatic ruler, since she lived in Egypt, has her name enclosed in a cartouche, and was buried at Thebes, presumably in the Valley of the Queens, like her daughter Ēse.⁴ By marrying her daughter Ēse, Ramesses III might have gained or at least strengthened his claims to the throne. For all we know she might even have been the wife of Ramesses III's father Setnakht and Ramesses III's step-mother, which would make her title 'King's mother' in Ramesses III's reign even more justified. Ramesses VI named his own daughter, borne to him by Queen Nub-khesbed,⁵ after his mother Ēse.

The inscriptions on a statuette of Ramesses VI in Cairo⁶ will have to be re-examined to see who is the 'god's wife, King's mother, . . . ' whom they mention, and whether it is permissible to read the name as Ḥabaḏilat, the name of this king's grandmother.

Since we have thus recognized Ramesses VI as a son of Ramesses III, there is nothing to prevent us from identifying the prince heading the representation of a series of the sons of Ramesses III at Medīnet Habu.⁷ The figures were cut under Ramesses III, but the titles and names of the princes were inserted later. This was done in two instalments. On the first occasion the first three figures were provided with legends; the first prince with the name of Ramesses in a cartouche, the second and third figures jointly with the name of Ramesses without a cartouche and the cartouches of Nebmaṣrē-miamūn and Ramesses-Amenḥikhopshef-neterḥekōne, that is Ramesses VI. Still later seven further figures received names, the first of these, that is the prince no. 4, the name of Sethikhopshef and the cartouches of Usimarē-akhenamūn and Ramesses-miamūn-Sethikhopshef, that is Ramesses VIII.

¹ The new *Assyrian Dictionary* of the Or. Inst. of the University of Chicago gives the meaning of *ḥabaṣillatu* as 'fresh shoot of a reed', but surely this cannot be the primary meaning, which must have been the name of a flower to be given to a woman. The sign  at the end of the name in the inscription of Dēr el-Bakhīt does not support the identification with the Semitic plant-name as Isidore Lévy thought. It is not a determinative, but it is generally supposed to be a writing of *m/r-ḥrw* 'true of voice' (see *Wb.* II, 17, 17 and 18). It occurs again after the name of Ēse, daughter of Ramesses VI, and her mother Nubkhesbed (Petrie, *Koptos*, pl. 19, 2) and often later. It is curious, however, that on the Koptos stela it is included in the cartouche, and *m/r-t-ḥrw*, written  or  in the ordinary way, follows outside the cartouche as well.

² Leps., *Denkm.*, III, 224, a, now almost completely destroyed, but the reading is confirmed by the remains of the corresponding inscription on the right jamb.

³ *Untersuchungen*, I, 63-64.

⁴ The tomb of Ēse (no. 51) is mentioned in Pap. Abbott 4, 16.

⁵ Petrie, *Koptos*, pl. 19, 2 (PM v, 129-30).

⁶ Legrain, *Statues et statuettes (CGC)*, II, no. 42153 (pl. XVI and p. 19); Scharff in *Studi in memoria di Ippolito Rosellini*, I, pl. XXXIII, 2.

⁷ Leps., *Denkm.*, III, 214a. b, and 214c.

It is therefore natural to see in the very first prince Ramesses the future king Ramesses IV, as was done by Petrie¹ and others before him despite the fact that no further distinctive cartouche was added to his name. Sethe² and Peet,³ to whom detailed discussions of these two series of princes are due, objected that since the mother of Ramesses VI, Ēse, in her tomb no. 51 was called '*King's mother*' but not '*King's wife*' she was not married to a king; they therefore decided that the prince no. 1 was the father of Ramesses VI, 'who never was king, but who, according to his son's belief, ought to have been. Consequently he [i.e. Ramesses VI] inserted him in the list with a cartouche, but could find no more specific name for him than Ramesses.'⁴ This line of reasoning has been accepted by Seele⁵ but becomes impossible once it has been recognized that Ramesses VI was a son of Ramesses III. The first prince must have also been a son of Ramesses III and there is no obstacle to seeing in him Ramesses IV, whose other name was not added because there was not room enough for it.

In a detailed discussion of representations of Ramesses III's sons throughout the reliefs at Medinet Habu, Seele⁶ has shown that their names were originally left blank, and he finds a very plausible explanation of this strange fact by assuming that the reliefs were copied from earlier scenes of Ramesses II. In two cases only names were added later, one being the list of princes with which we have dealt above. The second is a relief on the façade of the window of royal appearances⁷ representing two princes watching military games. The first of them had an uraeus added later on his forehead along with his name and titles '*King's son, commander-in-chief of the army, Ramesses, true of voice*'. In the temple of Ramesses III at Karnak,⁸ however, two princes are figured with their names: '*King's scribe, commander-in-chief of the army, King's beloved son of his body, Ramesses, true of voice*' and '*King's scribe, commander of horses, King's beloved son of his body, Ramesses-Amenhikhopshef, true of voice*'.

It is difficult to imagine how a simpler and more natural explanation could be suggested than that the second case at Medinet Habu represents the king's eldest son Ramesses, and the relief at Karnak the same Ramesses and the son next to him in seniority Ramesses-Amenhikhopshef, and that these are precisely those sons who later came to reign as Ramesses IV and Ramesses VI. In the list of princes they bear the same titles 'commander-in-chief of the army' and 'commander of horses' respectively and as kings their second cartouches (𐎓⁹ -name, that is, the names which they received at birth) are Ramesses⁹ and Ramesses-Amenhikhopshef, though this latter then always has the addition 'god and ruler of Heliopolis'.

Before they became kings they both had their tombs prepared in the Valley of

¹ *A History of Egypt*, III (3rd ed.), 137, 139.

² *Untersuchungen*, I, 62-63.

³ *JEA* 14, 55.

⁴ Peet, loc. cit. 55.

⁵ In *Ägyptologische Studien H. Grapow zum 70. Geburtstag gewidmet*, 303 ff.

⁶ Loc. cit. 299 ff.


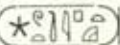
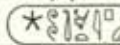
⁷ *Medinet Habu*, II, pl. 111, ll. 34-35; cf. Seele, loc. cit. 299.

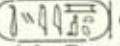
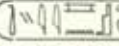
⁸ *Reliefs and Inscriptions at Karnak*, I, pl. 18, ll. 10 and 11. The inscriptions are contemporary with the relief, see Seele, loc. cit. 309.

⁹ Ramesses IV is often given this simple name of Ramesses without any addition, e.g. at Wādi Hammāmāt (Cuyat and Montet, no. 222), on a stela from Koptos in Cairo (*Rec. trav.* 11 (1889), 91), in the hypostyle hall at Karnak (Gauthier, *Le Livre des rois*, III, 183, XXIIIb), in the temple of Khons at Karnak (Gauthier, loc. cit. III, 184, XXIV, c and d), &c.

Queens: that of Ramesses was identified by Yoyotte¹ as tomb no. 53, while the well-known tomb no. 55 belonged to '. . . King's scribe, commander of horses of the place of chariotry of Usimarē-miamūn, King's son, Amenḥikhopshef'. Both must have been sons of Ēse, since Amenḥikhopshef was 'born of the god's wife, god's mother and great King's wife'² and in tomb no. 53 Yoyotte and I could just decipher that Ramesses was 'born of the great [King's] wife' too. That Ēse was a 'great king's wife' we know from the inscription on the statue of Ramesses III at Karnak,³ and that she was a 'god's wife', from the inscription from Dēr el-Bakhīt discussed earlier in this article.

The one who is missing in the list of princes at Medīnet Habu is Ramesses V, whose place ought to have been between Ramesses IV and Ramesses VI. That he was not admitted to the series was either because he was not recognized as a legitimate ruler later, when the cartouches were added, or because he was not a son of Ramesses III, whose sons the series purported to represent.

This latter explanation seems to be the correct one, since Ramesses V can be shown to have been very likely a son of Ramesses IV. On the thickness of a door in the temple of Khons at Karnak a  is represented⁴ before Khons, but the figure and name of the king standing opposite her are destroyed. This part of the temple was built and decorated by Ramesses IV, but Ramesses III is also represented and named several times in a near-by room. It has, however, been pointed out⁵ that this *Tintipt* must be the same person as the owner of tomb no. 74 in the Valley of the Queens,⁶ a queen whose name is written  or  and similarly.⁷ She was '(great) King's wife' and 'great King's mother'.⁸ She could not be the wife of Ramesses III, who was Ēse, nor the wife of Ramesses VI, who was Nubkhesbed, but she might well have been the wife of Ramesses IV, as was hesitatingly assumed by Gauthier.⁹ She might then have been the mother of Ramesses V, who is, in fact, absent from the list of Ramesses III's sons at Medīnet Habu.

The only one among the queens of the first half of the Twentieth Dynasty who remains to be placed is the 'great King's wife and King's mother'  or . She is once represented sitting behind and adored together with Setnakht on a stela from Abydos,¹⁰ so she cannot be other than the wife of this king. Since, however, she is represented again on two blocks, also from Abydos, following, without any title, Ramesses III¹¹ we can legitimately conclude that she was the mother of Ramesses III.

¹ See his note in the present volume of *JEA*, p. 26.

² Sander-Hansen, *Das Gottestweib des Amun*, 47, note 3, quoting Colin Campbell, *Two Theban Princes*, 114.

³ Leps., *Denkm.*, III, 207g.

⁴ Maspero, *Rec. trav.* 32, 88 (PM II, 83).

⁵ Sander-Hansen, *Das Gottestweib des Amun*, 8, n. 1.

⁶ PM I, 48.

⁷ See, for variants of her name and her titles, Champollion, *Not. descr.* I, 403. Her name occurs as Duaten-tōpet in the title of the usurper of the Theban tomb no. 346, who is 'overseer of the women of the Royal Harem of Duaten-tōpet' (Davies-Gardiner, *Seven Private Tombs at Kurnah*, pl. 40 and p. 56).

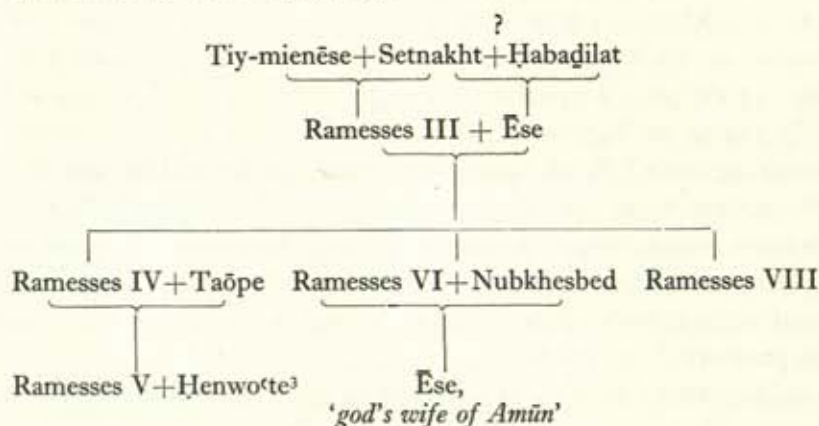
⁸ In the second room, east wall, north of the door, col. 2 of the text in vertical columns and, similarly, Champollion, *Not. descr.* I, 403 (lower 2nd from right), contrary to Sander-Hansen's statement, *op. cit.* 8, n. 1.

⁹ *Le Livre des rois*, III, 190.

¹⁰ Cairo JE 20395, Mariette, *Abydos*, II, 52 (left) (PM v, 51).

¹¹ One in Cairo JE 36339, Petrie *Abydos*, II, pl. 35, (8), the other at Brussels E 584. For bibliography of both see PM v, 43.

Since it seems established that Ramesses III was the son of Setnakht¹ and Ramesses IV the son of Ramesses III,² we can reconstruct the family tree of the first half of the Twentieth Dynasty in the following way:



It is perhaps worth pointing out that with the mutual relationship of Ramesses IV, V, and VI, as accepted here, agree their ages as far as they could be established during the unwrapping of their mummies. Ramesses IV was 'at least fifty years and probably more',⁴ as one would expect of the son of a king who reigned for over thirty years. Ramesses VI was 'probably not beyond middle age',⁵ which would also accord well for a younger brother of Ramesses IV. On the contrary Ramesses V, who is believed to have been a son of Ramesses IV, was found to be 'much younger than his predecessor'.⁶

Postscript

My article was already in the Editor's hands when I received the May/July number of *Bibliotheca Orientalis*, vol. 14, with Nims's review (pp. 136-9) of *Ägyptologische Forschungen*, where he devotes more than two columns to Seele's article on the family of Ramesses III and offers a discussion of the two lists of princes from Medīnet Habu. Nims's remarks are of considerable importance in view of the fact that he is one of the epigraphers responsible for plates 299 and 301 containing these lists, which are to appear in the forthcoming fifth volume of *Medinet Habu*. I have let my article stand, and prefer to add this postscript, pointing out that Nims agrees with me that the first prince Ramesses represents Ramesses IV. He also shows that this figure was inscribed by

¹ Of the references given by Peet, *JEA* 14, 57, n. 2, only P.Harris 75, 10-76, 2 is a safe proof of this parentage.

² The evidence for this is enumerated by Seele in *Ägyptologische Studien*, &c., 307, n. 4 (P.Harris 22, 3-4; 23, 2; 42, 4-5; 56b, 3; 66b, 5; 79, 5. In all these passages speaking of Ramesses IV, his predecessor Ramesses III calls him 'my son').

³ Her estates are mentioned in P.Wilbour as well as those of another possible Queen of Ramesses V Twertenro (Gardiner, *The Wilbour Papyrus*, II, 157).

⁴ G. Elliot Smith, *Bull. de l'Inst. d'Égypte*, 5th ser., 1 (1907), 60. According to the French résumé (loc. cit. 66) the mummy offers 'l'aspect d'un homme d'âge moyen'.

⁵ Loc. cit. 63. According to *The Royal Mummies* (CGC), 94, 'apparently middle-aged—probably older than Ramesses V, but younger than Ramesses IV'.


⁶ Loc. cit. 61. 'Mort dans la force de l'âge', says the French résumé, on p. 65.










Ramesses IV himself and refers also to the examples of this Pharaoh's name in its simple form, Ramesses, as I have done above, p. 34, n. 9. Moreover, he draws attention to the titulary of Ramesses IV below the two processions of princes, an eloquent feature to which I did not pay enough attention when taking my notes at Medinet Habu in 1926. Everyone agrees that the second and third figures represent one person only, namely Ramesses VI. Nims also thinks that the fourth figure is Ramesses VIII, but while I have suggested that this Ramesses VIII was a further son of Ramesses III (in which case the intervening Ramesses VII was perhaps a son of Ramesses VI), Nims thinks that Ramesses VIII was a son of Ramesses VI, and was followed on the throne by Ramesses VII. The chronological order of the two kings whom we call Ramesses VII and VIII is of course hypothetical, and there would be no objection to the reversal suggested by Nims did it not raise the fundamental problem of the intended identity of the procession as a whole. It seemed to me that by proving that the first three figures were two sons of Ramesses III, and by admitting that the fourth (Ramesses VIII) was also his son, there was nothing to prevent the conclusion that the remaining persons in the procession also represented sons of the Temple's founder, as was undoubtedly the intention when these uninscribed figures were originally carved. Nims is now inclined to view them as sons of Ramesses VI (as did Sethe and Peet), but I do not feel tempted to subscribe to this proposition until more evidence is forthcoming to support it.

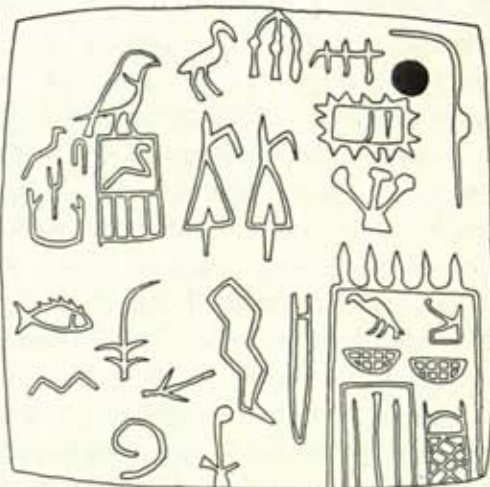
Through the kindness of the Director of the Epigraphic Survey of the Oriental Institute of the University of Chicago at Luxor Miss Moss has received two photographs of the inscription of the west statue of Ramesses III in the temple of Mut at Karnak (neg. nos. 10981-2) which she has allowed me to examine. The cartouche of Queen Ēse has suffered considerably since Lepsius' time, especially in its lower part. The photographs show that Lepsius' reading is essentially correct, though there is enough room for [⌒] in [⌒] and for the required [⌒] in [⌒], while in Lepsius' copy the ⌒ coalesced with the base of [⌒] above it. The most important thing, however, is that Lepsius' ⌒ can still be seen with sufficient clearness, so that it is not Lepsius' copy but the ancient sculptor himself who needs emendation. An ancient mistake of ⌒ instead of ⌒ to transcribe ⌒ is unexpected, but understandable in an unfamiliar Semitic name. The ⌒ properly corresponds to [⌒] or [⌒] (see Burchardt, *Die altkanaan. Fremdworte*, 1, § 107, for details), but never to ⌒. A Semitic *חבשלֹת is without etymology.

THE PERSONAL NAME OF KING SERPENT

By SIR ALAN GARDINER

As a firm believer in the theory set forth by Grdseloff, *Ann. Serv.* 44, 279 foll., or alternatively in some theory closely resembling it, I am convinced that the  following the *nbty*-title on the famous tablet from Naḳāda is really the draughtsboard *mn* and that it consequently spells the name of the defunct Menes. If the reader will now consult H. Müller, *Die formale Entwicklung der Titulatur der ägyptischen Könige*, 54 foll., he will find that in every early example of the *nbty*-title or the combined *insibiya* and *nbty* title what immediately follows is a name of the king; to Müller's evidence may be added *Šn* as the name of the Pharaoh Ḳaṣa¹ and the priestlike figure representing Semempses on the Cairo fragment of the Palermo stone.² It seems to follow that the like must be true of the tablet of King Serpent discovered by Emery in Tomb No. 3504 at Saḳḳara.³

But what can be the reading of the two structures following the *nbty*-title within the palace-like building here described as being visited or having been erected (for *šḥr*?)⁴? There can be no doubt as to what is represented by the object below the -crown here replacing the uraeus of the *nbty*-title; it is an excellent and exact picture of the *Pr-mw* or *Pr-nzr* which was the sanctuary, or according to Sethe the palace, of the Lower Egyptian capital at Buto; correspondently the structure beneath the vulture must be a rough delineation of the *Pr-wr*, although it admittedly bears no pictorial resemblance to the  regularly employed for the Hieraconpolite edifice, the sole point of similarity lying in the abnormal size of the sign as used hieroglyphically, compare the examples collected in Emery, *Hor-Aḥa*, p. 99. These two signs juxtaposed are frequent in the Pyramid Texts in the writing of the familiar dual word   *itrty*; without preceding phonetic signs as in King Serpent's tablet, *Pyr.* 256a. I conclude, then, that the personal name of the Serpent-king, to give it a fictitious English vocalization, was Iterty. This is the more probable, since the later sources show *it-* to have been the two first elements of the name; the Abydos list has  , the Turin Canon perhaps   , while Manetho, lumping together the second, third, and fourth kings of Dyn. I, uses Athōthis for the three.











¹ *Ann. Serv.* 44, 281, fig. 28.

² *Loc. cit.* 284.

³ *Great Tombs of the First Dynasty*, II, 102, fig. 105; also *ibid.*, pl. 35.

⁴ A similar use of *šḥr* in *Royal Tombs*, II, 3A, no. 5; in the two scenes in the Step Pyramid reproduced in *JEA* 30, pl. 3, figs. 3. 4; also in Palermo Stone, rt. 3, nos. 1. 9, where Schäfer rendered 'Aufenthalt in'.

But would it not be very strange to have a dual word as the name of a king? And what could it mean? To the first of these questions a very satisfactory answer can be given. In *Pyr.* 577 we read the punning utterance 'Horus has caused the gods to unite with thee, that they may fraternize with thee (*śn-śn ir-k*) in thy name of Two-Stelae (*Śnwti?*) and that they may not repudiate thee (*twr-śn tw*)     in thy name of Two-Sides (*Itrti*); so in T.P.M.N. As Sethe has pointed out,¹ one of the scribes of N has substituted *rn-śn* for *rn-k*, making the suffix refer to the gods (*Pyr.* 1830); doubtless he was embarrassed, as I imagine my readers to have been, at the employment of a dual word as the name of a particular king. But now I must justify my translation of that name as 'Two-Sides'. In Mercer's Excursus XX on the word *itrt* he has quoted the note (*JEA* 30, 27, n. 3) in which I maintained, as against Sethe, that the  and  depicted primitive temples or shrines, not royal palaces; but unfortunately he, like everyone else so far as I can see, has disregarded my contention with regard to the word *itrt*. After reviewing the evidence with care, I am more persuaded than ever that this word means fundamentally a 'row' or 'side' or 'line', not any sort of single building. In secular contexts 'row' is often a convenient rendering, see *Wb.* 1, 148, 6. 7; in the court of the Vizier the high officials are ranged *m itrt*y in two rows or lines in front of him.² I explained the determinatives in the Pyramid Texts as due to recollection of the two opposite lines of shrines at the Sed-festival, the Upper Egyptian shrines having the appearance  and the Lower Egyptian ones the appearance . Various passages in the Pyramid Texts demand this meaning. For example, in *Pyr.* 1297 (cf. 1369, 2017), where we read 'The Upper Egyptian *itrt* and the Lower Egyptian *itrt* came to him bowing down', it is obviously not alone the Hieraconpolite and Butite deities who perform this act of homage, and still less the actual buildings in which they dwell; the reference is rather to the gods of the two halves of the country strung out each in a line or a row in the Upper and Lower Egyptian directions; in the article above quoted I found it convenient to render *itrt* by 'conclave'. Similarly when the dual *itrti* is followed by *pt* 'sky' or *ih*t 'horizon' (*Pyr.* 757, 1541, 1862); in the first of these passages we might perhaps render 'Thou findest Rē standing and waiting for thee; he takes hold of thy hand and guides thee in the two sides of heaven'; Mercer, following Sethe, translates 'he leads thee into the double *itrt*-palace of heaven', which to my mind conveys but little sense. I presume, therefore, that on the tablet of the Serpent-king, whether his postulated name *Iterty* is to be taken simply as 'Two-Sides' or as a *nisbe* 'He of the Two Sides', the reference will have been to his dominion over both Upper and Lower Egypt.

I am well aware that my argumentation in this matter shows regrettable gaps and awkwardnesses; for example, I have omitted to mention that Ranke, in his *Ägyptische Personennamen*, 189, 25-26; 190, 1 foll., lists several compound proper names introduced by the *nbt*y group; these, however, occur at rather later periods, and in very different contexts. If it be conceded, as I hope it will be, that the two buildings following the *nbt*y-title on King Serpent's tablet are to be read as a royal name, then surely it will be found impossible to draw any other conclusion than has here been suggested.

¹ *Übersetzung und Kommentar*, III, pp. 83-84.

² *Urk.* IV, 1104.


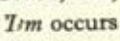
THE LAND OF YAM

By D. M. DIXON

OUR most important source on the land of Yam¹ is the autobiography of Harkhuf,² who made four journeys thither. For the first³ of these no details are furnished beyond the statement that the whole enterprise, including the journey there and back and the stay in Yam, took seven months.

On his second journey Harkhuf set out 'on the Elephantine road'.⁴ Now hitherto it has been generally assumed that this phrase means 'on the road which leads from Elephantine', and that Elephantine was therefore the starting-point of the journey.⁵ However, a few lines later Harkhuf states that on his third journey he 'set out from . . . [a place the name of which is partially destroyed] upon the oasis road',⁶ which can only mean that he set forth from this place on the road which led to (or via) the oasis.⁷ On this analogy *hr wst sbw* should mean 'on the road which leads to (or via) Elephantine'. This would imply that Harkhuf's starting-point lay farther north than Elephantine.⁸

Now after he had returned to Egypt and buried the body of his father Mekhu, Sabny sailed downstream to Memphis⁹ with the products which Mekhu had obtained and presented them to Pharaoh.¹⁰ Similarly, at the conclusion of his successful punitive campaigns in Nubia, Pepinakhte took a large number of the prisoners with him to the Residence (*Hnw*).¹¹ Harkhuf himself mentions the Yamite escort which was returning

¹ Apart from this inscription, Yam (*Im* > *Ym*) is mentioned, along with other localities, in the Dahshūr Decree of Phiops I (*Urk.* I, 209, 16), on an unpublished statuette in the Cairo Museum (*Bull. Inst. fr.* 52, 173), and in the inscription of Weni (*Urk.* I, 101, 14; 109, 1). The older and standard writing of the name is  which occurs for the first time in the reign of Phiops I (*Urk.* I, 209, 16) and thereafter frequently (*Urk.* I, 101, 14; 124, 11; 125, 13, 15; 126, 7, 10, 11; 127, 5, 11; 128, 8; 129, 4, 12). The variant  *Ym* occurs for the first and only time in the reign of Merenrē I (*Urk.* I, 109, 1).

In the *Ächtungstexte*, dating from the M.K., appear the names of two unidentified Nubian localities *Ym-n-ts* and *Ym-wtn* (?) (Posener, *Princes et pays d'Asie et de Nubie* (1940), 59; cf. E. Edel, *Ägyptologische Studien*, herausg. v. O. Firchow, Berlin, 1955, 67, 70). Whether or not these are to be connected with the Sixth Dynasty *Ym* is not clear. In any case it is doubtful whether they could be used as evidence for the location of Yam in the O.K. The most that can be said is that *Ym* appears to have survived as an element in the names of two unidentified localities in Nubia which may have included parts, or formed part, of the territory occupied by the O.K. Yam.

² On the transliteration and meaning of the name, *Hr-hwy-f* 'Horus, he protects', see Edel, *Äg. Stud.* 51, n. 1.

³ *Urk.* I, 124, 9-15.


⁴ *Urk.* I, 125, 1.

⁵ E.g. Breasted, *Anc. Rec.* I, p. 153, note h; Yoyotte, *Bull. Inst. fr.* 52, 174 ('la seconde mission qui partit . . . "par la route d'Éléphantine", emprunte la piste qui, à partir du Ier nome, se dirige vers Kourkour'); 176 ('une autre [route] partait d'Éléphantine'). This view is based on the fact that it was at Elephantine that the expedition-leaders had their homes and were buried. A mass of correspondence belonging to them was found on the island.

⁶ *Urk.* I, 125, 14.

⁷ The interpretation 'I set forth from . . . on the road which leads from the oasis' would give no satisfactory sense.

⁸ It would obviously be absurd to assume that Harkhuf, on his way to Yam, set out 'on the road to Elephantine' from a point south of it.

⁹  *Inb* 'the Wall'.

¹⁰ *Urk.* I, 139, 3-4.

¹¹ *Urk.* I, 133, 14-15; 134, 6-10.

with him to the Residence after his third journey,¹ and at the conclusion of the fourth, he is ordered by the young Phiops II to 'come downstream to the Residence immediately'.² Memphis was thus certainly the terminus of these expeditions, and Edel³



FIG. 1

not unreasonably suggests that the expedition-leaders may have remained in Memphis at the conclusion of their journeys until again called upon to undertake a fresh mission, especially if no great length of time elapsed between the journeys. It is thus very likely that Harkhuf had travelled north to the royal residence at Memphis at the conclusion

¹ *Urk.* I, 127, 5-6. 12.

² *Urk.* 1, 129, 15; cf. 130, 16.

³ *Ag. Stud.* 64.

of his first expedition to report to the king the results of his mission, to present the products he had acquired, and to receive further instructions.

He will then have set forth from Memphis¹ along the banks of the Nile 'on the road which led to Elephantine',² his home, where perhaps he tarried awhile before continuing his journey to Yam. No details, however, are given of this outward route beyond Elephantine.

Thus Elephantine, though certainly a point on Harkhuf's route to Yam, was not the starting-point. The eight months, therefore, given as the duration of this mission, and likewise the seven months for the first expedition, represent, as Edel³ has rightly seen, the time taken to travel from *Memphis* to Yam and back, including the stay in that country.

I cannot, however, agree with Edel that this fact (i.e. that Memphis was the starting-point) enables us to utilize these indications of time to determine the approximate location of Yam.⁴ Edel represents the whole journey from Memphis to Yam and back by $2x$ and takes as an average day's march for a caravan of several hundred asses, 15 km. Ignoring any rest days *en route*, and allowing ten days for Harkhuf's stay in Yam, he obtains the equation $\frac{2x}{15} + 10 = 210$, this last figure corresponding to the duration of the first mission, viz. seven months of thirty days. Hence x (i.e. the distance one way only, Memphis to Yam) = 1,500 km. Now 1,500 km. south from Memphis, presumably as the river flows, would place Yam in the vicinity of Sedeinga, about 250 km. south of Wādi Ḥalfa. The figure 240 days (eight months) for the second mission, gives x as 1,725 km., which would point to a position beyond the Third Cataract near Dongola el-Ordi. Between Sedeinga and Dongola el-Ordi lay the trading-centre at Kerma, which Edel claims was already in existence during the Sixth Dynasty.

Now quite apart from the correctness or otherwise of this last point, Edel's method is open to criticism. In the first place, the correctness of the estimate 1,500 km. obviously depends upon (a) the estimate of ten days for the length of the stay in Yam being reasonably near the mark; (b) that all the remaining 200 days of the first mission were spent on the march; and (c) that the estimated rate of 15 km. per day during these 200 days is also reasonably close. Now the ten-day period allotted for the stay in Yam can hardly be anything more than a guess, while the assumption that all the remaining days were marching days seems quite unjustifiable, even for the purpose of an approximation. Harkhuf must surely have halted *en route*, if only to rest and water his asses, especially

¹ It is possible that a careless sculptor has omitted some words after *pr-n(i)* in *Urk.* 1, 125, 1. Was the text meant to read *pr-n(i) [m + place-name = Memphis] hr wst sbw?* 'I set forth [from Memphis] on the road which leads to [or 'via'] Elephantine.' On the other hand, in the parallel statement, *Urk.* 1, 125, 14, the place-name governed by *m* is certainly not Memphis. It is possible, as Edel suggests (*Äg. Stud.* 64), that reference to Memphis as the point of departure was omitted in both cases as being self-evident.

² The only puzzling thing is why Harkhuf should have chosen to travel from Memphis along the bank of the Nile when it would have been so much quicker and more convenient to have travelled by boat to Elephantine and there transferred on to asses the trade-goods brought from the Residence. However, there seems no getting away from the text; there is no doubt that *wst*, though it is also used with reference to travel by water (e.g. *Urk.* IV, 322, 7), here refers to land travel, especially in view of the parallel usage in *Urk.* 1, 125, 14.

³ *Äg. Stud.* 63-65.

⁴ *Ibid.* 66.

if the distance to Yam was anything like Edel's estimate. It is possible, too, as has been suggested above, that Ḥarkhuf, after travelling from Memphis, lingered awhile at his home in Elephantine. Consequently, in addition to the days spent in Yam, an unknown number must be deducted for halts *en route*. The estimated travel rate of 15 km. per day is presumably based on that of a modern caravan of asses, unless this figure too is a guess.¹ However, even if Edel's figure of 3,000 km.² as the distance from Memphis to Yam and back is retained, we have no right to assume that the distances Ḥarkhuf covered from Memphis to Yam and from Yam to Memphis were even approximately the same, since we have little idea what routes he followed. If his routes of advance and return were not identical, one of them will almost certainly have been longer than the other. Hence the division by two of the estimated total distance travelled by Ḥarkhuf from Memphis to Yam and back will not give even the approximate distance of Yam from Memphis unless we know that he had followed the same route there and back.³ Finally, even if this point, too, be ignored and 1,500 km., as the river flows, be accepted as the approximate distance of Yam from Memphis, it would place the former in the vicinity of Sedeinga or Dongola el-Ordi only if we could be certain that the route Ḥarkhuf took had followed the course of the river for the whole of these 1,500 km. If he did not follow the Nile all the way, but deviated from it across the desert, he may quite easily have covered 1,500 km. without getting anything like as far south as Sedeinga. On the second journey we can be certain that Ḥarkhuf followed the Nile only as far south as Elephantine, while on the third he followed the river only as far as Abydos, from whence he crossed the desert to Khārgah; and from thence his route is uncertain.

I think that enough has now been said to show once and for all that the indications of time mentioned in the accounts of Ḥarkhuf's first and second journey are by themselves quite useless for determining even the approximate location of Yam.⁴ However, I would emphasize that it is not the location of Yam in the Kerma region that I query, though I think it improbable, so much as the method employed to locate it there.

Ḥarkhuf's route on his return from his second journey to Yam led through *Mchr*, *Trrz*, and *Irtt* in the land of *Irrtt*.⁵ We are also told that he passed through ('descended

¹ As far as I know, the travel rate of a caravan of asses is not mentioned by any ancient source, and one presumes Edel's estimate is not based upon the rate of progress of a camel caravan. Cf. Säve-Söderbergh, *Ägypten und Nubien* (Lund, 1941), 19.

² I.e. 200 days at 15 km. per day.

³ In the case of the third mission, at least, we know that he did not do so. On the outward journey he travelled via Khārgah, but returned via *Zftw*, *Irrtt*, and *Wrtwt*, which lay in the Nile Valley.

⁴ Cf. Säve-Söderbergh, op. cit. 17-18. The most that can be said of them is that 7-8 months seems to have been the average duration of a mission to Yam and back. On his third journey, for which no time is mentioned, Ḥarkhuf thought it necessary to write to the Pharaoh informing him that he had gone after the Yamite chief (*Urk.* 1, 126, 7 ff.). His reason for so doing was presumably because this incident had caused a delay which would prolong the duration of the mission beyond the normal 7-8 months. There are certainly no grounds for Edel's assumption that the second journey 'seiner längeren Dauer entsprechend ein gutes Stück weiter nach Süden führte als der erste' (*Äg. Stud.* 67). He is probably right in regarding the *Imwtw* mentioned in Phiope II's letter to Ḥarkhuf (*Urk.* 1, 128, 12) as a district within Yam, but it does not follow from the absence of previous mention that it was the furthestmost point which Ḥarkhuf reached in that land.

⁵ *Urk.* 1, 125, 2-3. So Yoyotte, *Bull. Inst. fr.* 52, 176, followed by Edel, *Äg. Stud.* 70, 71-72. For this use of apposition see Gardiner, *Eg. Gramm.* § 90, 3.

from') *Zṣṭw* as well as *Irrt* and explored both countries,¹ which at that time were under the rule of a single chieftain.² To these points we shall recur later.

On his third journey Harkhuf, as already stated, set out from a place the name of which is partially destroyed, on a road which led to (or via) an oasis.³ Now we have seen that the starting-point of nearly⁴ all these journeys to the south was Memphis and there is little doubt, therefore, that it was from Memphis that Harkhuf set out on his third mission. However, the partially destroyed name of the point at which he embarked on the road to the oasis is not that of Memphis, so that if he originally set out from this city and travelled along the Nile, he must at some point have left the valley in order to take the oasis route. It is important to establish approximately where this point was, since only if we know this can we gain any idea which oasis is meant.


Now the destroyed name is certainly not Elephantine, so that there can be no question of the oasis route being that from Elephantine to (or via) Kurkur or Dunkul.⁵ Sethe,⁶ who is followed by Yoyotte,⁷ restored the broken sign as the nome-sign of the 7th Upper Egyptian nome of Diospolis Parva. According to Edel's recent collation,⁸ the sign is a very developed or debased form⁹ of the nome-sign of the 8th Upper Egyptian nome of Abydos. Since from both these points roads lead to the Oasis of Khārgah, whichever of the two readings one accepts,¹⁰ there seems little doubt that by the 'oasis road' is meant that to Khārgah.¹¹

Why Harkhuf should have travelled via Khārgah is not clear. It has been suggested that he may have experienced some difficulty with the chief of *Zṣṭw* and *Irrt* during his return from the second journey, and therefore deemed it expedient to make a detour to avoid these countries.¹² However, even if this were so, it would not by itself explain his decision to travel via Khārgah, for if his object in taking an oasis route had been solely to by-pass *Zṣṭw* and *Irrt*, which on any view must have lain well south of the First Cataract (see above), he need not have left the Nile Valley as far north as the latitude of Khārgah, but could quite safely have followed the river as far south as Elephantine, as previously, and then have made a detour via the oases of Kurkur and Dunkul.¹³ The fact that Harkhuf did not follow this route (see above) suggests that

¹ *Urk.* I, 125, 8-9, *wbi* + country as direct object (cf. *Urk.* I, 208, 15; 209, 1). Harkhuf's explorations, however, could not have been very thorough, since even after he had 'explored' *Zṣṭw* and *Irrt*, he still needed a guide amid 'the paths of the ridges (*ṭswt*) of *Irrt*' on his return from the third journey (*Urk.* I, 127, 9).

² *Urk.* I, 125, 8.

³ *Urk.* I, 125, 14.

⁴ Sabny received the news of his father's death while he was at Elephantine, , (*Urk.* I, 136, 8) and travelled from thence to recover the body. But this was doubtless an exceptional case (cf. Edel, op. cit. 64, n. 3; Breasted, *Anc. Rec.* I, § 367 and note d).

⁵ This seems generally agreed (cf. Yoyotte, *Bull. Inst. fr.* 52, 174; Edel, op. cit. 63), though Sāve-Söderbergh, op. cit. 28, thinks Harkhuf did travel via Kurkur and Dunkul.

⁶ *Urk.* I, (2nd edn., 1933), 125, note a.

⁷ *Bull. Inst. fr.* 52, 174.

⁸ Winter of 1950-1; *Äg. Stud.* 62-63, 73-75.

⁹ '... einer allerdings sehr fortschrittlichen Form.'

¹⁰ Edel, op. cit. 63, gives good reasons for preferring Abydos.

¹¹ This fact is additional proof, if any is needed, that the starting-point of the journey was Memphis. Had Elephantine been Harkhuf's point of departure, he would never have travelled from thence first to Khārgah and then to Yam.

¹² Cf. Sāve-Söderbergh, op. cit. 19, 28.

¹³ Unless, of course, these oases were also subject to the authority of the chief of *Zṣṭw* and *Irrt*. But of this there is no evidence.

the visit to Khārgah may not have been entirely, if at all, connected with Yam. Possibly Harkhuf had been entrusted with the transaction of some official business at Khārgah, which could be conveniently disposed of on his way to Yam. However that may be, from the point when Harkhuf reaches Khārgah, his route is uncertain. There are, of course, various possibilities, but the discussion of these is best deferred until later.

When Harkhuf eventually arrived in Yam, he found, no doubt much to his annoyance, that the ruler of that country had gone off¹ on an expedition 'to smite the land of *Tmḥ* as far as the western corner of heaven'.² Leaving his asses and most of his goods at the chief's residence, Harkhuf, probably with a few companions and a Yamite guide, set out after him.³

Now, one may ask, why did Harkhuf think it necessary to pursue the chief of Yam, and what did he do when he found him? Though the formal consent, and possibly also the presence, of the Yamite chieftain may have been necessary before bartering could commence, this by itself would not have been sufficient to send Harkhuf chasing after him. One cannot easily imagine the representative of Pharaoh, arriving after a long and doubtless tiring journey at a miserable native village and being told that the chief was away, promptly going after him merely in order to tell him that he had arrived to trade. He would have been much more likely to have sent one of the chief's own servants after him to inform his lord of the arrival of the Egyptian traders. The Egyptians in the meantime would make themselves comfortable and await the chieftain's arrival. Hence one cannot agree with Edel that Harkhuf's purpose in following the chieftain was merely 'um seine Ankunft anzuzeigen'.⁴ There must have been some more pressing reason.

One can well appreciate Harkhuf's alarm on being told on his arrival that the chieftain had gone to war. Harkhuf may well have feared that the outbreak of inter-tribal hostilities would have the effect of closing the whole area to future Egyptian commercial activity. It might also have an adverse effect on the flow of recruits from Yam and *Tmḥ*, for in the previous reign these lands were among those which had furnished soldiers to Weni's army.⁵ Harkhuf would thus have every reason to follow the chief in order to avert, if possible, such a calamity.⁶

Hölscher,⁷ though he has rightly perceived the purpose of Harkhuf's pursuit of the

¹ *Urk.* 1, 3rd m. sing. Old Perfective, lit. 'I found the chief of Yam, he having gone etc.' (cf. Edel, op. cit. 52, 67-68; Gardiner, *AEO* 1, 116*). Harkhuf did not meet the chief of Yam while he (Harkhuf) was on the road, as Yoyotte thinks (*Bull. Inst. fr.* 52, 174, 177). Quite apart from the grammatical understanding of the passage, such an interpretation would accord ill with Harkhuf's next statement, that he set out for *Tmḥ* after the chief (*Urk.* 1, 126, 2). He did not 'accompany' him to *Tmḥ* as Yoyotte says (op. cit. 52, 177). What possible purpose could there have been in his doing so? Even had Harkhuf met the Yamite force on its way to *Tmḥ*, he would surely have dissuaded the chieftain from his project there and then.

² *Urk.* 1, 125, 17-126, 1. Does the 'western corner of heaven' really designate a locality? The expression 'to smite someone as far as the western corner of heaven' does not seem to occur elsewhere. Is it possible that we have here a slang expression, comparable perhaps to the English 'to hit someone for six', i.e. beyond the boundary? Harkhuf would then be quoting, from the mouth of one of the chief's retainers who had remained in Yam, the colourful expression with which his lord had announced his intention of chastising *Tmḥ*. If the expression was a piece of Yamite slang, this might explain why we do not come across it elsewhere in Egyptian.

³ *Urk.* 1, 126, 2.

⁴ *Äg. Stud.* 54.

⁵ *Urk.* 1, 101, 14, 16.

⁶ It is to be noted, incidentally, that Harkhuf does not say that he actually reached *Tmḥ*. Presumably, therefore, he succeeded in overtaking the chief of Yam before the latter reached his objective.

⁷ *Libyer und Ägypter*, 25, n. 5.

chief, has, in common with most other commentators, failed to realize the meaning of *šhtp* in this passage. Now it follows from the fact that Ḥarkhuf wished to avoid any conflict, that he himself would not have used force against the chieftain of Yam. He may, in any case, not have been in a position to do so. The fact that, on his return from Yam via *Irrtt*, *Ztṯw*, and *Wṣwt*, the ruler of those countries appears to have been deterred from attacking him only by the presence of an escorting force of Yamites,¹ would indicate that Ḥarkhuf's own party was comparatively small and, hampered as it was by pack-animals² and goods, would have been powerless to deal with any large force. Moreover, the fact that the Yamites provided Ḥarkhuf with an escort for the return journey³ is proof of peaceful, friendly relations, for an unwilling escort conscripted from a recently chastised people would have been more of a menace than a help to the Egyptians.⁴ The sense, therefore, of *šhtp* cannot be that Ḥarkhuf 'pacified' the chief of Yam by force or 'reduced him to subjection'.

Though Edel has realized this, according to him the meaning of *šhtp* here is 'zufriedenstellen (durch gute Bezahlung)', and he thinks that this refers, not to Ḥarkhuf's action upon overtaking the chief of Yam, but to the barter which took place in Yam after the chieftain's return with Ḥarkhuf, whose sole object in going after him had been to announce his arrival.⁵ Edel is led to this interpretation by his understanding of the next statement, that the chief 'praised all the gods for the sovereign's sake'. This idiom, *dw ntr(w) n X* 'to praise the god(s) for someone', is a regular expression for 'to thank someone' and is especially used of returning thanks for satisfactory payment. Hence Edel concludes that Ḥarkhuf, by generous payment for the goods he received, so satisfied ('zufriedenstellte') the chief of Yam that the latter 'gave thanks to the sovereign'.⁶

My only criticism of this interpretation is that by dissociating *šhtp* from Ḥarkhuf's action on overtaking the chief, we are left with no satisfactory reason for his pursuit of the Yamite. It seems to me more likely that *šhtp* refers to Ḥarkhuf's treatment of the chief on the road to *Tmḥ*. However, it seems possible to connect *šhtp* and the following sentence both with this incident and the trading in Yam, with slight modification of Edel's rendering. Thus Ḥarkhuf, on hearing of the chief's intentions, set out after him and on overtaking him he 'calmed him down' (*šhtp*) and reasoned with him, perhaps pointing out that he would be working against his own interests in stirring up war, since this would result in the interruption of commerce. Doubtless realizing the wisdom of this argument, and perhaps gratified by a presentation of sample goods, the chief 'gave thanks to the sovereign' and accompanied Ḥarkhuf back to Yam.⁷ There Ḥarkhuf will have ensured that bartering took place to the chief's satisfaction.⁸

¹ *Urk.* 1, 127, 4 ff. ² *Urk.* 1, 126, 17. ³ *Urk.* 1, 127, 4-6. ⁴ Cf. *Äg. Stud.* 54. ⁵ *Ibid.* 53-54.

⁶ *Ibid.* 72. It is the 'sovereign' whom the Yamite thanks, not Ḥarkhuf. Trade with these countries was a royal monopoly (Säve-Söderbergh, *op. cit.* 20; Edel, *op. cit.* 54).

⁷ It is not quite clear from the text at what point, whether before or after his pursuit of the Yamite, Ḥarkhuf dispatched his message to the Pharaoh (*Urk.* 1, 126, 7-10 = Edel, *op. cit.* 54, Abb. 1, Z. 1-2) informing him of the incident. Apparently it was after his return to Yam with the chief. Ḥarkhuf may then have realized that the incident would mean prolonging the duration of the mission beyond the normal 7-8 months (cf. p. 43, n. 4) and accordingly deemed it advisable to inform Merenrē of the circumstances. It is also possible, however, that he sent the message before setting out after the chief.

⁸ *Urk.* 1, 126, 11 = Edel, *op. cit.* 54, Abb. 1, Z. 2; i.e. *šhtp* in connexion with the bartering which took place in Yam = 'satisfy'.

At the conclusion of his business, Harkhuf, accompanied by an escorting force of Yamites, set out on the return journey to Egypt via the countries of *Irrtt*, *Zstw*, and *Wstwt*. Through the first two, at least, of these¹ he had travelled on the return from his previous mission.

Now at that time *Zstw* and *Irrtt* were under the rule of one chief, whence it has been concluded that they bordered one another. Furthermore, *Wstwt* must have adjoined them, or at least one of them, since on the return from this third expedition Harkhuf found a single chieftain ruling over *Irrtt*, *Zstw*, and *Wstwt*. Finally, since *Wstwt* was the last of the three to join the coalition, it cannot have lain between the other two.²

Now it is clear from its occurrence in Old Kingdom inscriptions alongside other names which are certainly those of localities in Nubia, that *Wstwt* at this period designated a restricted portion only of Lower Nubia;³ and the inscription of Pepinakhte indicates that it lay north of *Irrtt*.⁴ Evidence for its exact extent, however, is lacking, but it seems very doubtful whether this can be determined on the basis of Middle Kingdom evidence, for by that period the name had evidently already been extended to cover Lower Nubia from Biggah at least as far south as Korosko.⁵

On the basis of a rock-inscription dating from the reign of Phiops I at Tumas,⁶ about 30 km. upstream from Korosko, which commemorates an official who had been sent thither to explore (*wbs*) *Irrtt*, it has been concluded that Tumas lay within the district so named.⁷

Since *Wstwt* stretched northwards from *Irrtt* probably as far as Biggah, *Zstw* must have lain either between *Irrtt* (Tumas) and *Wstwt*, or upstream of *Irrtt*. Now we do not know the northern boundary of *Irrtt* or the southern limit of *Wstwt*, and it is possible, therefore, that *Irrtt* stretched downstream from Tumas⁸ and had a common frontier with *Wstwt*, in which case *Zstw* must have lain south of Tumas. Even if Tumas be regarded as the northern limit of *Irrtt*, *Zstw* may still have lain upstream from that country if we assume that the space between Tumas and the southern frontier of *Wstwt* was too small to accommodate it.⁹

On the other hand, we do not know how large or small a country *Zstw* was. It is noteworthy that unlike *Irrtt*, which occurs frequently in the inscriptions of the Old Kingdom¹⁰ and was evidently a kingdom of some importance, *Zstw* is not mentioned

¹ Harkhuf may on his second journey also have passed through *Wstwt*, mention of which was accidentally omitted, cf. *Urk.* 1, 126, 15; 127, 4. ² Säve-Söderbergh, op. cit. 16. ³ Loc. cit.

⁴ *Urk.* 1, 133, 9-10: 'The majesty of my lord sent me to hack up *Wstwt* and *Irrtt*.' Pepinakhte's campaign was launched southwards from Egypt. Hence *Wstwt*, as the first country he reached, is mentioned first. In like manner, Harkhuf, travelling from south to north on his return, reached *Wstwt* last and hence it is mentioned last (*Urk.* 1, 126, 15; 127, 4). I think Säve-Söderbergh, op. cit. 14, is a little too sweeping in his statement that 'wir aus der Reihenfolge in der Aufzählungen der südlichen Gebiete nichts schließen können'.

⁵ On this see my Brief Communication in this volume of the *Journal*.

⁶ *Urk.* 1, 208-9.

⁷ So Säve-Söderbergh, op. cit. 15 (with references to previous discussions); Gardiner, *AEO* 1, 75*; Yoyotte, *Bull. Inst. fr.* 52, 176.

⁸ Especially if the *wstwt nt zstwt nt Irrtt* of *Urk.* 1, 127, 9 be identified as the stretch of hill country between Tumas and Medik (so Säve-Söderbergh, op. cit. 28-29; Yoyotte, loc. cit. 177 and n. 2; Edel, op. cit. 73, n. 2). On the other hand, *zstwt* would perhaps more suitably describe the broken, hilly country east of the Nile.

⁹ This is evidently the opinion of Säve-Söderbergh, op. cit. 16, cf. map.

¹⁰ *Urk.* 1, 101, 13; 109, 1; 110, 15; 111, 10; 125, 8; 126, 15; 127, 4; 133, 10; 208, 15; 209, 16.

farther south than the Second Cataract, for the desolate nature of the Baṭn el-Ḥagar, which commences a little south of Ḥalfa and stretches upstream for 100 miles,¹ precludes its location in that area;² and that it cannot at this time have lain south of the Baṭn el-Ḥagar seems clear from the circumstances that it furnished recruits and workmen to Egypt,³ and that its chief appeared at the First Cataract to do homage to Merenrē I.⁴

On the position of *Kṣw* we know nothing. However, I conclude from the fact that it is mentioned but once, and then only at the end of the list of *Nḥst*-lands, that it was a small, unimportant state, which, if it did lie in the valley, may have been either north or south of *Mḏt*, but, for the same reasons, almost certainly north of the Second Cataract.

Before considering now the location of Yam, let us review the data on this land. First, the negative points. No conclusion regarding the position of Yam can be drawn from the order in which it is mentioned, since this varies in every case.⁵ The products with which Ḥarkhuf returned from Yam likewise afford no guidance in locating that land, since Ḥarkhuf does not say that they were native to it; they may thus have been acquired from elsewhere.⁶ Even if they were native to Yam, the identification of some of them is uncertain, and the area of distribution of the remainder at that time is unknown. Finally, we have seen that the indications of time mentioned in the accounts of the first and second journeys are valueless for locating Yam.

Turning now to the positive data, the first point to be noticed is that the products enumerated were obtainable only in Yam,⁷ either because they were native only to that country or because its position was such as to enable it to corner these goods, if they came from further afield, before they could reach the other lands. Secondly, Yam was not a member of the *Zṯw*-*Ḥrrt*-*Wṣwt* coalition. Moreover, it was evidently capable of mustering a sufficiently imposing force to be able to escort Ḥarkhuf right through the coalition's territory and to overawe its chief into aiding the Egyptians.⁸ Thirdly, not only was Yam able to overawe the coalition, but it was also able to treat Egypt quite coolly. Thus at the beginning of the reign of Merenrē I, the chiefs of *Mḏt*, *Ḥrrt* and *Wṣwt* travelled to the First Cataract to do homage and obeisance to the Pharaoh.⁹ There is no mention, however, of the chief of Yam, who evidently felt under

¹ Cf. H. E. Hurst, *The Nile* (1952), 74.

² One could conceivably use the fact that workmen and troops from *Mḏt* were employed by the Egyptians to support the location of this land in the Baṭn el-Ḥagar. The argument would then be that it was so difficult to scrape a living from this desolate tract that its inhabitants were driven to seek service in Egypt. However, it seems one would have to draw a similar conclusion from the appearance in the Egyptian service of Yamite troops. But the rest of the data on Yam cited above, pp. 49 ff., would not support such a conclusion.

³ *Urk.* I, 101, 14; 109, 2.

⁴ *Urk.* I, 110, 15; 111, 10. Cf. *AEO* I, 74*.

⁵ Cf. Säve-Söderbergh, op. cit. 14-15.

⁶ The pygmy obtained on the fourth expedition is explicitly stated to have come from the land of the Horizon-dwellers (*Urk.* I, 128, 15-16; cf. also Kuentz, *Bull. Inst. fr.* 17). Ḥarkhuf, incidentally, does not say that he himself had been to that land.

⁷ There is, it is true, a reference in the inscription of Sabny to *sntr*, elephant-tusks, etc., apparently from elsewhere in Nubia (*Urk.* I, 137, 9-10), but the text at this point is too battered for any definite conclusions to be drawn.

⁸ *Urk.* I, 127, 4-9.

⁹ *Urk.* I, 110-11.

no obligation to put in an appearance. Finally, Yam supplied soldiers and workmen to Egypt.¹ From these facts it may be inferred that Yam was, comparatively speaking, a rich, powerful, and populous state, and possibly quite a large one. Moreover, unlike *Wꜣwt* and *Irrꜣt*, Yam was probably so situated that it felt able safely to hold aloof from any acknowledgement of Egyptian power.² In fact, the impression one gains is that throughout their dealings with Yam, the Egyptians, whatever feelings of contempt they may have entertained privately for the Yamites,³ conducted themselves as though dealing with equals;⁴ and this could surely only have been because they realized the impracticability of exerting pressure on Yam. But in what direction from Egypt did Yam lie and how far afield?

Yoyotte⁵ would locate Yam in the oasis of Dunkul, since, like Yam, it is accessible from Elephantine, Khārgah, and Tumas, which lay within *Irrꜣt*. However, though such a concentric position for Yam would accord reasonably well with the above-cited data, it is not free from difficulties. In the first place, Dunkul, at the present time at least, is rather a miserable little oasis, certainly not a likely spot for the rich and populous country which we may envisage Yam to have been. One could, of course, retort that conditions may not have been the same then as now. However that may be, there remains the time difficulty. If Elephantine had been Ḥarkhuf's starting-point, as Yoyotte thinks, it is incredible that a mission from thence to Dunkul and back should have taken 7–8 months to accomplish, even allowing for a longish stay there and possible delays *en route*. Even with Memphis as the point of departure, the difficulty is hardly less.⁶

One has also to consider the question of the location of *Tꜣ-Tmḥ*, which Yoyotte would place in the oasis of Dakhlah. Now the most convenient route from Dunkul to Dakhlah is via Khārgah, but had the Yamite force taken this road, it would surely have met Ḥarkhuf's party on the way; unless, of course, it had passed through Khārgah just before Ḥarkhuf arrived there. But had this been the case, the passage through any part of the oasis of a large force of fighting men would hardly have passed unnoticed by the inhabitants, who would surely have informed Ḥarkhuf on his arrival there. As it was, however, Ḥarkhuf did not learn that an expedition had been dispatched to *Tmḥ* until he arrived in Yam. Any other route from Dunkul to Dakhlah would have involved a lengthy desert march—even as the crow flies the distance between the two places is about 350 km.—during which the force would have been dependent for water on small, scattered wells; while the only means of transport available would be asses, which

¹ *Urk.* I, 101, 14; 109, 1.

² Later, however, in Merenrē's reign, when they had been banded together, even *Wꜣwt* and *Irrꜣt* apparently felt strong enough to assert themselves by demanding transit-dues (*tongo*) from the Egyptians.

³ Cf. *Urk.* I, 126, 11: *hꜣ Tmꜣ pf* 'that wretched chief of Yam'. For this force of *pf* see Gardiner, *Eg. Gramm.* § 112. Ḥarkhuf was probably still feeling a little sore at the thought of the time and energy wasted in pursuing the Yamite on the road to *Tmḥ*.

⁴ Such also was undoubtedly the case in the trade with Punt, both at this time and later. Cf. Kees, *Kulturgeschichte*, 124: 'man handelt, gibt selbst Geschenke und bewirtet die zum Empfang erscheinenden Landeshäuptlinge. Damit erwirkte man die Erlaubnis [italics mine], nach Belieben Ebenholz zu schlagen, Weihrauch zu sammeln . . .'

⁵ *Bull. Inst. fr.* 52, 176.

⁶ Cf. Edel, *op. cit.* 68.

cannot go more than a couple of days without water.¹ Finally, there is no positive evidence that Dakhlah, or any of the other oases for that matter, was inhabited by *Tmḥ*-people. There is, admittedly, a Nineteenth Dynasty inscription in which a *Ti-Tmḥ* is mentioned which was accessible from Sebuah,² but the most one can reasonably infer from this is that settlements of *Tmḥ*-people were to be found in one or more of the oases of the western desert at the period in question. It could certainly not be used as evidence for their presence here in the Old Kingdom.³

Now the data on Yam listed above could also support its location in the Nile Valley. Among other points, we noted that the products with which Harkhuf returned from Yam were apparently obtainable only in that land, whether they were native to it or not; that Yam was not a member of the Nubian coalition; and that its chief did not appear at the First Cataract to do homage to the Pharaoh. Accordingly, if Yam did occupy a stretch of the Nile Valley, it will, in view of these facts, probably have been the farthermost upstream of all the Nubian lands.⁴ Junker⁵ would identify Yam with the Maḥass country, and a similar location (north or south of Kerma) is proposed by Edel.⁶ Now Dar Maḥass or the Kerma area can be reached by land⁷ from Memphis either by following the course of the Nile most of the way, or, if one wishes to avoid Lower Nubia, by leaving the river at Elephantine and marching through the desert via the oases of Kurkur and Dunkul to Selima, and from thence to the Nile again at Sakiet el-'Abd.⁸ It is by one of these routes that Harkhuf would have had to reach Dar Maḥass or the Kerma area after leaving Elephantine on the second mission.

A quicker route leaves the Nile at Asyūṭ or Abydos, crosses the desert to Khārgah, and from thence runs south via El-Sheb as far as Selima. This route, part of the so-called Forty-days Road (Darb el-Arbatin), though very arduous, was chosen in preference to the Nile by the Arab caravans travelling to Darfur;⁹ and it is presumably the route Harkhuf would have had to take on his third journey, on which he reached Yam from Khārgah.

¹ Cf. Sāve-Söderbergh, *op. cit.* 19. It is true, of course, that the Egyptian expeditions, which must themselves have been rather primitive (*Bull. Inst. fr.* 52, 178, n. 1), covered considerably more than 350 km. on their journeys from Memphis to Yam. But even so, whether Yam lay in Dunkul or somewhere in the Nile Valley, for the greater part of the journey thither the Egyptians would have been able to travel along the river (or in the case of Harkhuf's third journey, through the well-watered Khārgah depression) and be thus assured of water-supplies. There could certainly have been no question of Egyptian caravans of hundreds of asses undertaking long desert marches, cf. above. One must remember, too, that the Yamite expedition to *Ti-Tmḥ* was a military one. It would not have been sufficient, therefore, merely for it to arrive there with perhaps a quarter of its troops having perished *en route*.

² Yoyotte, *Bull. Inst. fr.* 52, 177; id. *Bull. Soc. franç. d'Égyptologie*, 6, 9-14 (I have not had access to the latter). Cf. Edel, *op. cit.* 68.

³ The theory (Hölscher, *Libyer und Ägypten*, 49) of a south-to-north movement of *Tmḥw* in the period between the Old and Middle Kingdoms lacks definite evidence.

⁴ Contra Gardiner (*AEO* 1, 75*; II, 271*), who inverts the order of *Mḏ* and *Tm*, placing the latter farther downstream.

⁵ *Ermenne*, 39; cf. Sāve-Söderbergh, *op. cit.* 17. Junker's work was not accessible to me.

⁶ *Op. cit.* 66-67.

⁷ There is no suggestion that Harkhuf travelled any part of the way to Yam by river. In any case, on the Nile from Halfa to Kerma, a distance of about 250 miles, there is no continuous navigation (Hurst, *The Nile*, 73-74; cf. Reisner, *Sudan Notes and Records*, 12 (2), 147).

⁸ From Sakiet el-'Abd to Selima is a three days' journey by camel (*ibid.* 9 (2), 37).

⁹ *Ibid.* 12 (1), 64.

Now Harkhuf returned ('descended') from his second mission to Yam via ('from', *m*) 'the district of the house of the chief of *Zstw* and *Irrtt*',¹ which lay in the Nile Valley. Accordingly, if Yam lay in the Maḥass-Kerma region, he may have followed the Nile all the way from thence to the chief's residence. On the other hand, it is possible to leave the river at Sakiet el-ʿAbd, travel to Selima, and continue from thence through the desert. Had Harkhuf taken this route, he would have had to deviate from it at some point and descend into the valley somewhere north of the Second Cataract. This, apparently, is what Edel thinks Harkhuf did, for he renders *hṛ m* in *Urk.* 1, 125, 8 as 'herabsteigen in'² (an expression frequently used of descending into the Nile Valley from the higher deserts on either side),³ thus implying that prior to going down into the valley, Harkhuf had been travelling from Yam through the desert.⁴ However, the necessity of procuring regular and adequate supplies of water for his 300 asses makes it unlikely that Harkhuf can have travelled any considerable distance through the desert.⁵ If, then, Yam had lain in the Maḥass-Kerma region, the only sure means of getting there and back would have been by keeping fairly close to the Nile for most of the journey.

Apart, however, from the question of the routes to Maḥass or Kerma, the fact that Yam, like *Wswt*, *Irrtt*, and *Mds*, supplied Egypt with both troops and labourers, militates against its location so far south. Thus it is hard to believe that for the simple job of cutting and hauling acacia-wood in *Wswt*,⁶ for which ample man-power was available near at hand, workmen should have been summoned from as far afield as the Kerma area.⁷ Edel⁸ counters this objection by supposing Yam to have *begun* farther north, but this view is based merely on the assumption that the *Imsw* mentioned in the account of Harkhuf's fourth journey⁹ was the farthestmost point reached in Yam. This we have already seen¹⁰ cannot be proved, and since Edel's location of *Tj-Tmh* in the 'Steppengebiet westlich des Niltals zwischen Kerma und Alt-Dongola (Dongola

¹ *Urk.* 1, 125, 8. According to Edel (op. cit. 61), he will also have travelled via this place on his return from the third mission.

² Op. cit. 72: 'Ich stieg (aber) herab in die Gegend des Hauses des Herrschers von *Zstw* und *Irrtt*, nachdem ich diese Fremdländer erkundet hatte.' Cf. his translation of [*hṛ m*] in *Urk.* 1, 126, [12]: ['stieg ich herab nach . . .']. 'To go down, descend into', however, is usually *hṛ r* in Middle Egyptian, as also in O.K. inscriptions (e.g. *Urk.* 1, 52, 6; 53, 6; 83, 9; 130, 6 [= to go down to a boat, i.e. embark]; 137, 7; 149, 17; 189, 8; 199, 12 [to descend into the tomb]; 296, 1). However, *hṛ m* does occur with this meaning in the Pyramid Texts, though here too *hṛ r* is also found.

³ E.g. *Urk.* 1, 149, 17 [read $\bar{\text{r}}$ for $\bar{\text{r}}$]; de Buck, *Eg. Readingbook*, 1, 88, 15; further examples *Wb.* 11, 472, 17.

⁴ Edel's translation, according to which Harkhuf would have descended into the Nile Valley only after he had 'explored' (*wbi*) *Zstw* and *Irrtt*, would also imply that the territory of these two states extended from the valley into the higher desert.

⁵ When asses are employed on long journeys, it is only in mixed caravans with camels, which carry fodder and water for them (Säve-Söderbergh, op. cit. 19; cf. Burckhardt, *Travels in Nubia*, London, 1819, 163 ff.).

⁶ *Urk.* 1, 108, 14 ff.

⁷ It is not a question here of the Egyptians employing a number of odd Yamites who had drifted north to seek work, as was probably the case with the Yamite soldiers who enlisted in Weni's army (Säve-Söderbergh, op. cit. 26), for the inscription makes it quite clear that the Yamite labour was an organized force provided by the chief of Yam himself (*Urk.* 1, 109, 1-2), though in view of our information on Yam (pp. 49 f.), the statement that the chief himself took part in the work is probably not to be taken literally.

⁸ Op. cit. 67.

⁹ *Urk.* 1, 128, 12.

¹⁰ Cf. p. 43, n. 4.

el-Agusa)¹ depends upon his location of Yam in the region north or south of Kerma, it too must be rejected.

It seems to me that if Yam is to be located in the Nile Valley, it is unlikely to have lain farther south than the Second Cataract. Indeed, in view of the uncertainty regarding the exact location and extent of all the Nubian lands, and the possibility that two of them, *Kꜣꜣw* and *Mdꜣ*, lay in the desert, Yam may have lain considerably farther north than the Second Cataract and have adjoined *Zꜣꜣw* upstream, in which case it will have occupied the stretch of the valley from the unknown southern limit of this land perhaps as far as the neighbourhood of Halfa. If so, Harkhuf would have reached it on his second mission, after leaving Elephantine, either by travelling through the desert via Kurkur to Dunkul, from whence he could reach the valley again at Tumas and then follow the course of the river; or, more probably, by continuing along the bank of the Nile, thus passing through *Wꜣꜣst*, *Irrꜣt*, and *Zꜣꜣw*. Though only the last two are mentioned in the account of the return, the omission of *Wꜣꜣst* is probably purely accidental in view of its occurrence in the account of the next journey. The omission of any reference at all to either *Mdꜣ* or *Kꜣꜣw* could be taken as an indication that they did not lie on Harkhuf's route through the valley and hence probably lay in the desert to the east of it.

On the third mission Harkhuf reached Yam via Khārgah. Accordingly, he would probably have travelled from thence via Dunkul to the Nile at Tumas, and then continued along the river bank. The fact that Tumas lay within *Irrꜣt*, upstream of which lay *Zꜣꜣw*, with both of which lands Harkhuf at some stage in his travels apparently experienced some difficulty, in no way tells against this view. All commentators, it is true, have assumed that the difficulties which Harkhuf is presumed to have encountered arose in the course of the return from his second mission and that he was therefore anxious to avoid *Zꜣꜣw* and *Irrꜣt* on the next journey. This view, however, is based merely on the fact that on the third mission Harkhuf chose to travel to Yam via Khārgah, the inference being that his purpose in so doing was to by-pass *Zꜣꜣw* and *Irrꜣt*. However, we have already seen that had this indeed been his object, he could quite safely have accomplished it without travelling via Khārgah, and that the reasons which impelled him to travel via this oasis may have been in no way connected with the attitude of *Zꜣꜣw* and *Irrꜣt*. The difficulties, therefore, which Harkhuf encountered with the chieftain of these lands may have arisen, not during the second mission, but in the course of the outward journey to Yam on the third. Hence Harkhuf's precaution to provide himself with a Yamite escort before venturing to return through these lands.

It must be admitted, however, that the location of Yam in the Nile Valley north of the Second Cataract also has its difficulties. In the first place, the evidence for the location of *Mdꜣ* and *Kꜣꜣw* in the desert is very slender. One might also question whether it could reasonably be said of a country whose southern limit lay at Halfa, that its location was such that it could feel safely able to hold aloof from acknowledgement of Egypt (a point which could also be raised in the case of the location of Yam in Dunkul). It is worth recalling that the Egyptians had penetrated as far upstream as the vicinity


¹ Op. cit. 68.

of Wādi Ḥalfa as early as the First Dynasty.¹ It might also be objected to this location that if Yam had occupied a portion of the valley northward from Ḥalfa, Ḥarkhuf would have been more likely to have travelled thither and back by boat, for navigation as far as Ḥalfa presents no problems.

Finally, one has to consider the location of *Tj-Tmh*. An expedition from the Ḥalfa region to Khārgah² or Dakhlah would presumably follow the Nile as far as Tumas and from thence cross the desert via Dunkul. However, it would be rather an ambitious undertaking, even for the powerful Yamites, and Gardiner³ rightly doubts whether it would have been within their capacity. In any case, the same difficulties would arise as with the location of Yam in Dunkul, viz. that the Yamite expedition would have encountered Ḥarkhuf's party on the way. For a force to have marched across the desert from Ḥalfa to Dakhlah seems out of the question. The only other oasis of reasonable size accessible from the Ḥalfa area, namely Selima, is unsuitable.⁴

To sum up: wherever Yam lay, it was probably not south of the 22nd north parallel. It is impossible, however, with the information at present available, to come to a more definite conclusion than this, and until further evidence is forthcoming, it seems preferable to leave the question open.

Postscript

Mr. R. O. Faulkner suggests that *šbw* in *Urk.* I, 125, 1, where it is written without the hill-country determinative , does not mean 'Elephantine' but 'ivory'. Hence he would render 'I set forth on the Ivory Road', meaning on the road upon which ivory, etc., from the south travelled north to Egypt.

This is not impossible, but it is not clear what one route, more than any other, would have been designated the 'Ivory Road' or at what point Ḥarkhuf embarked on it. One thinks, of course, of that leading south from the 'Ivory-' or 'Elephant-town', *šbw*, Elephantine. But another more famous route by which ivory, *inter alia*, arrived from the south and south-west, and which could therefore just as appropriately have been so designated, was the Darb el-Arba'in (*Sudan Notes*, 12 (1), 63 ff.), whose Nile terminus was at Asyūt. It is unlikely, however, assuming for the moment the identity of the 'Ivory Road' with the Darb el-Arba'in, that Ḥarkhuf would have embarked on it at Asyūt, for the Asyūt-Khārgah portion of the route is the worst stretch of all, even for camels (cf. H. J. L. Beadnell, *An Egyptian Oasis*, London, 1909, 33-34). He would have been more likely to have set forth on it from Khārgah, having crossed thither from Abydos, as on the third mission.

However, there is nothing to suggest that Ḥarkhuf touched Khārgah on the second

¹ *JEA* 36, 28-30 and pl. 10.

² Khārgah, however, seems excluded as a possible location for *Tj-Tmh*, since it is already designated as the 'Oasis' (*Urk.* I, 125, 14).

³ *AEO* I, 116*.

⁴ Leach (*Sudan Notes*, 9 (2), 42) notes that 'one of the most remarkable things about Selima which makes it perhaps unique among places in the Sudan is the fact that, though it contains water, palms, several other forms of vegetation, and salt, no Arab, tribe or individual, or other class of native claims, or apparently has ever claimed, any form of rights there. This alone is enough to make it certain that the Oasis cannot be worth much.'

mission or followed any part of the Arba'in road. Indeed, unless Yam had lain in the valley somewhere south of the Baṭn el-Ḥagar, which is improbable (pp. 53 f.), he would have had no occasion to use it on any of his journeys, apart from the third, when he followed the short stretch of it which runs through the Khārgah depression.

If, then, the translation 'ivory road' be accepted, the route in question would appear to be that leading from Elephantine. However, the fact that Ḥarkhuf travelled on the 'ivory road' which led from Elephantine in no way tells against the view that Memphis was his original point of departure; nor does it bring us any nearer to locating Yam.

The final revision of the foregoing paper has brought to light a slight inconsistency which had earlier escaped my notice. In discussing the position of *Wst*, *Irrt*, and *Zstw*, the order in which they are mentioned in *Urk.* I, 126, 15 and 127, 4 was cited (p. 47, n. 4) in support of the location of *Wst* north of *Irrt*. On p. 48, n. 1, however, the order in the same passages is rejected as evidence for the intermediate location of *Zstw*. This inconsistency, however, in no way affects our conclusion (p. 48) that *Zstw* did not lie in between *Wst* and *Irrt* but south (upstream) of the latter.

THE BREAD AND BEER PROBLEMS OF THE MOSCOW MATHEMATICAL PAPYRUS¹

By CHARLES F. NIMS

OF the twenty problems in the Moscow Mathematical Papyrus,² eleven are concerned with bread, beer, and grain. Eight of these contain a notation in regard to $bš$ and bnr .³ These notations seem to have been a source of difficulty to the original scribe, and they have confused his translators and interpreters. The present study attempts to clarify several matters in regard to these problems. I believe that ten of the eleven calculations in the class noted deal in one way or another with the values or prices of the commodities mentioned. I believe as well that the correct mathematical interpretation of these problems gives further support to the definition of $bš$ that I suggested in an earlier study.

Whereas the Rhind Mathematical Papyrus groups together problems of the same kind, and usually gives the method followed in working out the results, the Moscow papyrus has no arrangement and gives almost nothing of the manner of calculation.⁴ Moreover, its scribe was quite inconsistent in his writings of the same signs; he often left out parts of a problem and introduced numbers into the calculations without explanation. In some instances he certainly made errors, and several of his examples are still not understood. Also disconcerting is his mixture of $hekat$ notations and pure numbers.⁵

Seven of the eight problems with the $bš$ - bnr notation have the formula $\frac{3}{4} n bš$; $n bnr$, though most omit the second n .⁶ Included in this number is the fragmentary problem 5, which, as restored by Struve, I believe correctly, is a greatly abbreviated version of problem 8. The handling of this formula in these two problems illustrates the difficulties which the Moscow papyrus presents.

In ancient Egyptian mathematical calculations the indication of division is 'you are

¹ An abridged version of this paper was read at the XXIVth International Congress of Orientalists, Munich.

² W. W. Struve, *Mathematischer Papyrus des Staatlich Museums der Schönen Künste in Moskau*, Berlin, 1930. Reviewed by Peet, *JEA* 17, 154-60.

³ The $bš$ - bnr problems dealing with amounts of bread and beer to be made from U.E. barley are 9, 13, 22, and 24; with the exchange of bread for beer, 5 and 8; with the exchange of beer for emmer, 16, and with the making of beer alone, 12. Problem 15 deals with bread only, problem 20 with the exchange of emmer bread for U.E. barley, and problem 21 apparently is concerned with the average value of offering-cakes.

⁴ Peet, *The Rhind Mathematical Papyrus*, London, 1923; Chace et al., *The Rhind Mathematical Papyrus*, 2 vols., Oberlin, 1927. Peet, 'Mathematics in ancient Egypt', *Bulletin of the John Rylands Library*, 15, no. 2 (July, 1931), p. 33, characterizes Rhind as 'showing some elementary idea arrangement' against 'the disgraceful chaos of the Moscow papyrus'. In his review of Struve, op. cit., he says: 'The difficulties of the papyrus are at times appalling. Middle Kingdom hieratic of a cursive type is never easy, and in the whole range of the literature of this period I know of no case where the scribe has been so criminally inconsequent in the forms of his signs. What is more, he was in some problems dealing with a faulty original or with an original he did not understand. The result is in some cases chaos.'

⁵ For the $hekat$ notations, see Gardiner, *Eg. Gramm.*³ § 266. 1.

⁶ For a list of these, see Struve, op. cit., p. 69, n. 2 and 3.

to reckon x to find y ', which indicates that the second number y is to be divided by the first number x , the answer being $\frac{y}{x}$. In problem 5, apparently the formula $\frac{3}{4} n b\dot{s}i n bnr$ was abbreviated to $\frac{3}{4}$. Thus col. vi, ll. 6-7, with restorations, reads, 'You are to reckon $\frac{3}{4}$ to find 1. $\frac{1}{2}$ results.' This is obviously nonsense as it stands, as the answer should be $1\frac{1}{3}$. In problem 8, col. x, ll. 6-7, the fuller writing has, 'You are to reckon $\frac{3}{4} n b\dot{s}i n bnr$ to find 1. $\frac{1}{2}$ results.' Mathematically, this is no clearer. The solution of the mathematical puzzle is found in a parallel section of problem 22, col. xli, ll. 1-3, 'You are to reckon $\frac{3}{4} n b\dot{s}i bnr$ to find 1. Now $\frac{3}{4} n b\dot{s}i n bnr$ is 2. $\frac{1}{2}$ results.'

Whatever this formula may mean, the consequence of its inclusion in problems is to double the amount of Upper Egyptian barley needed to make any given number of jugs of beer. Consideration of its meaning will be undertaken later.

Here it is well to remember that in ancient Egyptian bread and beer calculations the number of *ds*-jugs of beer or the number of loaves or cakes which are made from one *hekat* measure of grain is referred to as *psw*, perhaps literally meaning 'cooked', but practically the 'cooking ratio'. Thus when two *ds*-jugs of beer are made from 1 *hekat* of barley, each has a *psw* of 2, and when 20 loaves of bread are made from one *hekat*, each has a *psw* of 20.

There follow some suggestions in regard to the difficulties in the problems. In problem 24 the given data are 15 *hekat* of U.E. barley to be made into 200 loaves of bread and 10 *ds*-jugs of beer, with the *psw* of the beer $\frac{1}{10}$ that of the bread, $\frac{3}{4} n b\dot{s}i bnr$. The problem is to find the *psw* of the bread and the beer. Struve believes that the datum, $\frac{3}{4} n b\dot{s}i bnr$, is then forgotten, as it does not appear to enter into the calculations. However, the final result is given as 100 loaves of bread with a *psw* of 20 and 10 jars of beer with a *psw* of 2. Now the *psw* is correct if the $\frac{3}{4} n b\dot{s}i bnr$ had been ignored, but the 100 loaves is correct for the amount of barley given if the *b $\dot{s}i$ -bnr* formula was used. Perhaps the error is in the number of loaves given in the first statement, where it should read '100'. Then in col. xliii, l. 6, there was omitted, 'You are to reckon 100 2 times. 200 results.' The rest of the problem then goes smoothly.

In problem 13, a shortened version of problem 9, the scribe did well until he reached the final result. Here, instead of having 6 jugs of beer of each of the three strengths, he has 12. Either he made an error in figuring, or else in his final calculation he forgot the effect of the *b $\dot{s}i$ -bnr* formula.

Problem 22 certainly appears to be utterly confused. The scribe is given 10 *hekat* of U.E. barley to make 100 loaves of bread, its *psw* unknown, and 10 jugs of beer of 2 *psw*, $\frac{3}{4} n b\dot{s}i bnr$. Of course, this amount of barley would be used up by the beer alone. In his calculations, having found the amount of barley used for 10 jugs of ordinary beer of 2 *psw*, 5 *hekat*, he should have multiplied this by 2. Instead, he subtracts this from the original 10 *hekat*, and divides the remaining 5 by 2 because of the $\frac{3}{4} n b\dot{s}i n bnr$! Having obtained the result of $2\frac{1}{2}$ *hekat*, he abandons his calculations entirely. The last portion of his example, col. xli, ll. 1-3, is an expanded version of a section of problem 8, col. x, l. 6-col. xi, l. 1. It may be that, having reached the figure of 5 *hekat* as the remainder, he borrowed the last lines in problem 22 from problem 8, thinking he had a parallel calculation.

Problem 20, though entitled 'Example of reckoning 1000 loaves of (*psw*)¹ 20', is actually concerned with value or price. The data given are, '1,000 loaves, of (*psw*) 20, in content completely from emmer', and the question to be answered, 'Let me know the emmer.' From the way the problem is solved, it is obvious that the full question was something like, 'Let me know (the value of) the emmer in U.E. barley.'

The first step in the calculation is, 'You are to reckon the part, (*psw*) 20, to find $2\frac{2}{3}$.' The word translated 'part' is written \square , with a light stroke across it. Struve at first believed that the scribe had mistaken *ph* *h*, 'this thousand', to mean *ph* and had written the determinative of that word, then crossed it out. In an additional note,² he quotes the suggestion of Neugebauer and Sethe that the sign was intended, and that the text be rearranged to give *ir·hr·k ir·k ph* (or *dnit*, so Sethe) *n* $2\frac{2}{3}$ *r gmt* 20, 'You are to reckon the division (or, part) of $2\frac{2}{3}$ to find 20.' Struve objects that the sign is crossed out, that there is no *n*, and that this is not the way division is done. (Also, if the suggestion is correctly quoted, it reverses the position of the two numbers.) He is certainly right in the second and third objections, though it is not so certain that the cross-stroke (which may be accidental) is a cancellation. I believe that Sethe was correct in his identification of the sign, but that it refers to that 'part' of the given data, '1,000 loaves of (*psw*) 20' which is '(*psw*) 20'. It is possible that the scribe, having so written, realized that *dnit* was redundant, and deleted it.

The figure $2\frac{2}{3}$ is introduced without explanation. Struve has seen correctly that it is the value ratio of emmer and barley; that is, 1 *hekat* of emmer is worth $2\frac{2}{3}$ *hekat* of U.E. barley. Thus the final answer is that 1,000 loaves of emmer bread of *psw* 20, using 50 *hekat* of emmer, are worth $133\frac{2}{3}$ *hekat* of U.E. barley. However, the scribe actually arrived at the answer by dividing 20 into $2\frac{2}{3}$, giving $\frac{2}{15}$ (written as $\frac{1}{5}$ of $\frac{2}{3}$), then finding $\frac{2}{15}$ of 1,000. He reduces the resulting pure number, $133\frac{2}{3}$, to the *hekat* notation, resulting in *hekat* $100 + 30 + 3 + \frac{1}{4} + \frac{1}{8} + \frac{1}{16} + \frac{1}{64} + 1$ *ro* + $\frac{2}{3}$ *ro*.³

Problem 16 is of a similar nature, but because of the difficulties of the script this was not recognized by either Struve or Peet.⁴ The given data are, 'A *ds*-jug of beer, of *psw* 2, $\frac{2}{3}$ *n bš* *bnr*, in exchange for $\text{𐀀}||| \dots \text{𐀀}||| \text{𐀀}$, emmer . . . at the value of $2\frac{2}{3}$.' The hieroglyphic transcription is of a section of col. xxxi, l. 4. The first sign in *bty* 'emmer' lacks the usual diagonal stroke at the upper right, but is paralleled by an example in P.Berlin 6619, vs., l. 2.⁵ The *t* and stroke is paralleled by the writing in P.Moscow, col. xxxvii, l. 2, as transcribed by Peet.⁶ What follows seems to be identical with the *hekat* notation 33, and has no meaning here. I think I can detect traces of other writing beneath the three tall strokes, and they may be the cancellation of an error.⁷ Otherwise, they must be due to another scribal confusion.

¹ The word *psw* here, instead of being either written out or abbreviated to the determinative, is indicated by a sign like a comma, often used in these problems. See also Chace, *Rhind*, II, problem 72, n. 4.

² Op. cit., in the Glossar, p. 189.

³ For a translation of the fractional parts of the *hekat* into modern equivalents, see the Addendum to this article.

⁴ Peet, *JEA* 17, 157, concludes, 'The whole example is clearly so corrupt that speculation as to what may have originally have stood there is almost valueless.'

⁵ *ZAS* 38, pl. 4, 2, opp. p. 138.

⁶ Peet, loc. cit.

⁷ A portion of the Moscow papyrus, at least, is a palimpsest; see the traces of earlier writing in col. xxxix. Thus it is always possible that various traces otherwise unexplained may belong to the earlier writing.

For the sign which I have translated 'value' Peet suggested the transcription \mathfrak{h} .¹ I have followed that suggested by Struve in his additional notes, \mathfrak{h} , though he believed it to be an abbreviation of *wdn* 'to be heavy'.² I take it rather as an abbreviation of $\mathfrak{h}\mathfrak{e}\mathfrak{k}\mathfrak{a}\mathfrak{t}$, found in P.Berlin 6619, vs., l. 4, where it almost certainly has the meaning of 'value' or similar. The value ratio between emmer and U.E. barley is thus the same as in problem 20.

The calculation proceeds without difficulty until the answer, $\frac{3}{8}$ (given as $\frac{1}{4} \frac{1}{8}$) in pure numbers, is reached. The scribe divides this by 1, which is given as a pure number and not as a *hekat* notation, as would have been proper. The final answer, in col. xxxii, l. 2, is \circ , written as an unusual ligature. This is, of course, (*hekat*) $\frac{1}{4} \frac{1}{8}$, but, largely because of the confusion about the nature of the problem, it has not been recognized. The basic situation in this problem is the exchange of beer, made of U.E. barley (though this is not actually named) according to the $\frac{3}{4} n b\bar{s} bnr$ formula, for emmer. Thus 1 *ds-jug* of this beer, using 1 *hekat* of U.E. barley, would buy $\frac{3}{8}$ *hekat* of emmer.

Another problem, no. 21, also deals with values. This is titled *šbn šnw*, with the data, '20, of the value of $\frac{1}{8}$ (*hekat*) and 40, of the value $\frac{1}{16}$ *hekat*'. The *hekat* notations are then changed to pure numbers, and the answer is that the *šbn* is $\frac{1}{16}$, as a pure number, apparently an error for $\frac{1}{32}$. Struve sees the title as a writing of *šbn snw*, 'mixture (?) of offering-cakes', while Peet thinks that *šbn* must have the meaning of 'average'.³ He believes the problem concerns '20 [loaves] containing each $\frac{1}{8}$ [*hekat*] of flour and 40 containing each $\frac{1}{16}$ [*hekat*]'. But the usual method of giving the grain or flour content is to indicate it by *psw*, which is not done here. Rather, it seems to be the average price or value which is sought, and perhaps the final answer is not changed to a *hekat* notation⁴ because the value could also be a ratio, as in problems 16 and 20.

P.Berlin 6619, vs., ll. 1-5, to which reference has been made, is a fragmentary problem of exchange. Several groups are lost at the beginning of each line, and in l. 5 the traces are too slight to give a basis for restoration. However, it is possible that all the data are preserved, and from these a solution can be suggested.

What remains can be translated thus: '(1) [Example of] exchange. If it is said to you (2) . . . with U.E. barley, *hkt knt*, 60; emmer, *hkt knt*, 15 (3) . . . this . . . with U.E. barley, *hekat*, 40; emmer, *hekat*, 60. Total⁵ (4) . . . Pray determine⁶ for me the value of the U.E. barley.' The meaning of $\mathfrak{h}\mathfrak{e}\mathfrak{k}\mathfrak{a}\mathfrak{t}$ *knt* is unknown.

I suggest that a man started with the amount of grain given in l. 2 and after making an exchange, or sale, ended with the amount given in l. 3. Using as a trial number the value ratio of emmer to U.E. barley used in the Moscow papyrus, $2\frac{2}{3} : 1$, the total value of the grain in l. 3 is $40 + (60 \times 2\frac{2}{3}) = 200$ *hekat* of U.E. barley. Taking only the numbers in l. 2, without regard to the meaning of *knt*, the result is $60 + (15 \times 2\frac{2}{3}) = 100$.

¹ Peet, loc. cit.

² Struve, op. cit., Glossar, p. 191, and Korrigenda, p. 198. Peet overlooked this correction.

³ Peet, *Bull. John Rylands Library*, 15, no. 2, pp. 21 f., and *JEA* 17, 157 f.

⁴ A fact which puzzled Peet, *JEA* 17, 158.

⁵ *Tot dmd.*

⁶ *Wpy* 'to divide, open, judge'. *Wb.* gives no mathematical usages of this verb.

Should we be following the correct clues, then *hkst knt* must be equal to the double *hekat* used in the Middle Kingdom.¹

It is possible that the problem was solved as follows. Each of the figures in l. 2 was doubled, assuming that *hkst knt*, perhaps meaning literally 'complete *hekat*', is a double *hekat*, giving in single *hekat* 120 of U.E. barley and 30 of emmer. Using the amounts in l. 3, and subtracting the smaller from the larger figure in each instance, 80 *hekat* of U.E. barley were exchanged for 30 of emmer. Dividing 80 by 30, the answer to the problem is $2\frac{2}{3}$ *hekat*, the value in U.E. barley of 1 *hekat* of emmer, 1 *hekat* of barley being worth $\frac{3}{8}$ *hekat* of emmer. Of course, such an interpretation is somewhat hypothetical, but it seems more than a coincidence that by the use of the value ratio of emmer and U.E. barley found in the Moscow papyrus,² there are obtained round numbers such as are frequently used in ancient Egyptian mathematical problems.³

In the problems of exchange, nos. 5 and 8 of bread for beer and no. 16 of beer for emmer, the commodity offered for exchange is first reduced to the equivalent in U.E. barley and given in the *hekat* notation. Problem 20, though it is not so called in the title, deals with the exchange of emmer bread for U.E. barley, and P. Berlin 6619, vs., ll. 1-5, with the exchange of emmer for U.E. barley. In problem 20 the value of the value of the various cakes is given in the *hekat* notation, with U.E. barley almost certainly understood.⁴ Thus these examples are actually concerned with buying and selling, and U.E. barley was the standard medium of exchange.⁵ It should be noted that the cost of labour and processing was not considered.

We must now examine more closely the formula $\frac{3}{4} n bš; n bnr$. As has been noted, this is said to 'equal 2'. However, in problem 12 a different formula is used, *bš; mi bnr* $2\frac{1}{6}$, with the expected *psw* at the end omitted. This problem would require $2\frac{1}{6}$ *hekat* of U.E. barley to make 2 *ds*-jugs of 2 *psw* instead of the usual 1 *hekat*.⁶

That *bnr* means 'dates' seems to be firmly established. Gardiner, after examining the evidence, rejected a conjecture that it 'is the name of some sweet tasting cereal'.⁷

¹ Gardiner, *Eg. Gramm.*³ p. 198, nn. 8. 9. 10.

² This assumes that the relative price of U.E. barley remained steady, or at least was so considered for the data of these problems. This certainly was untrue in Dyn. XX. Černý, *Archiv Orientalní*, 6, 174-6, gives the known material from this period. One case in the first half of the dynasty has the price ratio between emmer (*btj*) and barley (*it*) as 5:6 (nos. 6, 14), while from the last part of the period three documents give the ratio as 8:7, 1:1, and 1:1 (nos. 10, 17; 11, 18; and 12, 19). Within a short period under Ramesses VII (according to the numbering hitherto customary; see von Beckerath, *Tanis und Theben*, 87, and *Bibliotheca Orientalis*, xiv [1957], 138, urging that the chronological sequence of Ramesses-Sethikhopshef and Ramesses-Itamün should be reversed), the ratio varied from 8:5 to 1:3. In the late Pharaonic and Ptolemaic periods the price ratio between emmer and barley remained steady, at least for the purposes of the documents concerned, at 2:3; see Malinine, *Kemi*, II, p. 14 and pl. 2, where col. C is emmer and col. A is barley.

³ See *JEA* 15, 185, on the simplicity of geometrical problems.

⁴ The actual kind of grain here may have been of no interest to the scribe, as it is probable that ancient Egyptian mathematical problems are 'examples' which were to be followed in similar problems; see Peet, *Bull. John Rylands Library*, 15, no. 2, p. 23, and n. 2.

⁵ In the Rhind 'exchange problems', nos. 72-78, the given amount of bread and beer is first changed into *wdyt*, except in no. 74, where the bread is first changed into the necessary amount of U.E. barley, and the resulting quantity equated with the same amount of *wdyt*. This word, which is known only from Rhind, seems, from problem 82, to mean some sort of flour.

⁶ The problem actually deals with beer of 3 *psw*.

⁷ *AEO* I, 14; II, 225*.

But *bš* has remained a puzzle. Struve, by eliminating the cereals of known identification, came to the conclusion that it meant 'spelt',¹ and though Gardiner hesitates in accepting this, he shows that it must be a cereal.²

On certain Second Dynasty stelae *bš*(*r*) occurs along with wheat and dates.³ From the Third Dynasty through the Twelfth Dynasty *bš* appears as a label on granaries shown in funerary monuments, and from the Fourth Dynasty dates usually occur in the same scenes, often, but not always, in adjacent bins.⁴ Sir Alan notes that in one M.K. sarcophagus both are shown in one granary, as if mixed. This conclusion is somewhat less than certain, since wheat and *bš* are shown thus in the Third Dynasty,⁵ and we have one instance where wheat and dates seem to be shown together.⁶ The pairing of *bš* and dates occurs also in four accounts from the Kahun papyri⁷ and in an account in P.Boulaq 18.⁸

Gardiner has accepted Struve's studies showing that dates could be used as an ingredient for beer, and has called attention to the note at the beginning of the date account papyrus, Louvre 3326, 'memorandum concerning the dates that were given to the brewers, 40 sacks'.⁹ That *bš* was used for beer is definitely shown in Rhind 71, where it is the only ingredient except water. Two O.K. mastaba scenes have a legend over reapers, 'Beer for those who cut the *bš*(*r*).¹⁰ In the O.K. bread and beer scenes *bš*(*r*) is mentioned, without any certainty as to which process this cereal is assigned. However, in the legend *tšs* ('grinding') *bš*(*r*), where the verb is known only from these examples,¹⁰ it may be noteworthy that, according to the *Wb.* references, the cognate or derived verb *tš*(*r*), 'to crush', is used in connexion with grain only in the process of beer-making.¹¹

Struve believed that $\frac{3}{4} n bš n bnr$ means ' $\frac{3}{4}$ of *bš* (and) of *bnr*', and took the whole, with the addition of 2 *psw*, to indicate that ' $\frac{3}{4}$ of a jug of spelt-date beer equals 2 jugs of barley beer'. In what Peet rightly characterizes as 'an ingenious but wholly unconvincing appeal to Rhind 71', he pleads that normal *bš*-beer of 2 *psw* was so strong that it needed to be diluted to $\frac{3}{4}$ strength with water, and that the same was true of *bš*-date beer. Indeed, he seems to think that its diluted strength was still so strong that it was equal to barley beer made double strength, and that the $\frac{3}{4} n bš n bnr$ formula was used only as a label to indicate that the barley beer involved in these problems would be double the strength normally indicated by a given *psw*.¹²

¹ Struve, op. cit. 62 ff.

² AEO II, 223*.

³ Zaky Y. Saad, *Ceiling Stelae in Second Dynasty Tombs* (Ann. Serv., Cahier 21), nos. 15. 17. 18. 19, pls. 26. 21. 22. 23; Quibell, *Excavations at Sakkarah* (1912-1914). *Archaic Mastabas*, pl. 28, 1.

⁴ For the occurrences of *bnr* and *bš*, see the references in *Wb.* 1, Belegstellen, 461. 12-16; 478. 10; Struve, op. cit., pp. 60 f.; Gardiner, AEO II, 224* f.; Junker, Giza, I, 178 f. 188. 246; to which add Capart, *Chambre funéraire de la Sixième Dynastie*, 21; James, *Mastaba of Khentika*, 60; Ann. Serv. 16, 197. 211; Leps., *Denkm.* I, 103 a.

⁵ Junker, op. cit. 178.

⁶ Maspero, *Trois Années de Fouilles*, Mém. Miss. Arch. Fr. I, 201.

⁷ Griffith, *Hieratic Papyri from Kahun*, pls. 15, 66. 67; 18, 3. 4; 19, 3. 4.

⁸ ZÄS 57, 13**; 29, 13. 14.

⁹ Möller, *Hieratische Paläographie*, II, Taf. I.

¹⁰ Montet, *Scènes*, 231 ff.; Wreżinski, ZÄS 61, 1 ff.

¹¹ Bull. Inst. fr. 40, 69, S.I. 18, in the legend of the rescue of mankind from destruction.

¹² Struve, op. cit. 67 ff.

He translates *bš* *mi bnr* as 'spelt and something in the nature of dates', and says that beer made of *bš* and the date-substitute would have slightly less strength than *bš*-date beer. He gives the comparison thus, '1 jug spelt-date beer equals $2\frac{2}{3}$ jugs of barley beer', and '1 jug of spelt-and-date-substitute beer equals $2\frac{1}{6}$ jugs of barley beer'.¹ Unfortunately Struve, becoming absorbed in this explanation, has forgotten the actual problems. Those with $\frac{3}{4} n bš n bnr$ require only double the amount of barley for the beer involved, while that with *bš* *mi bnr* takes $2\frac{1}{6}$ times the normal amount. Thus, were Struve's contention right, the latter would be stronger than the former, not weaker.

Peet translated the formulae differently. For him $\frac{3}{4} n bš n bnr$ means 'attributing $\frac{3}{4}$ of the spelt to the dates', and indicates a mixture of 1 part of spelt and $\frac{3}{4}$ of a part of dates. He translates *bš* *mi bnr* as 'spelt like dates', indicating an equal amount of each. He follows Struve in his belief that these are merely labels, that 'just as one involves the doubling the amount of *šmr*-corn which goes to produce a *ds*-jug of a given *psw*, so the appearance of the other involves multiplying it by $2\frac{1}{6}$. Here once more it is necessary to urge that this interpretation does not involve the introduction of either spelt or dates into the beer, for the phrase, even if it had its origin in some kind of beer made from spelt and dates, merely serves as a guide to the strength of the beer made entirely with *šmr*-corn.'²

Both Struve and Peet believe that in the *bš*-*bnr* problems the normal *psw* notation indicating the strength of the beer is altered in an abnormal way. This very abnormality is a ground for suspicion that the interpretation is incorrect.

Sir Alan Gardiner, in his study of *bš* and *bnr* in connexion with the Onomasticon of Amenope, has a new approach. His conclusion is that 'the natural interpretation of the two indirect genitives is to take the formula to mean $\frac{3}{4}$ (*ds*?) of *bš* and $\frac{3}{4}$ of *bnr*; i.e., $1\frac{1}{2}$ *ds*, leaving $\frac{1}{2}$ *ds*—since this " $\frac{3}{4}$ " is said to equal 2 *ds*³—to some other ingredient, probably the very grain with which the problems are concerned.' 'Is it not the most plausible view that [the calculator] used his [U.E. barley] in the proportion of $\frac{1}{2}$ for beer-making and $\frac{3}{4}$ ($\frac{3}{4} + \frac{3}{4}$) given in exchange for *bš* and *bnr*, which were put into the beer?' He agrees with Peet that *bš* *mi bnr* 'can only mean *bš* and *bnr* in equal quantities'. Concerning the equivalent of this with $2\frac{1}{6}$, he says 'this new expression would be practically identical with the $\frac{3}{4}$ formula previously used, but the scribe has used the figure $2\frac{1}{6}$ to be able to present the strength of his beer as an integer'.⁴

This view is certainly an advance on the older ones. It recognizes that the *bš*-*bnr* formulae concern the content of the beer and indicate a value notation. But Gardiner still has the beer in the ' $\frac{3}{4}$ ' formula made with twice the amount of ingredients which the *psw* would indicate; i.e. $\frac{1}{2}$ *hekat* of U.E. barley, $\frac{3}{4}$ *hekat* of *bš*, and $\frac{3}{4}$ *hekat* of *bnr*, a total of 2 *hekat*, will produce only 2 *ds*-jugs of beer of 2 *psw* instead of 4 as would be normal. Since Rhind 71, where *bš* is the cereal out of which beer is made, uses the usual *psw* calculation, Gardiner's proposals do not solve this difficulty.

Before seeking to resolve this problem, attention must be given to the meaning of *bš*.

¹ Ibid. 91.

² JEA 17, 155 ff.

³ It must be emphasized that the *ds* throughout this quotation is Sir Alan's interpretation, and does not appear in the original.

⁴ AEO 11, 226* f.

Struve's definition, as 'spelt', accepted by Peet, is not possible; *spelt was never grown in ancient Egypt*. No botanist has ever discovered any trace of spelt among the ancient Egyptian vegetable remains,¹ and Prof. John Percival, who has written much on wheat culture, states that spelt-wheat 'is a comparatively modern kind, quite unknown to the ancient people of Egypt'.² Other authorities agree.

Several years ago I suggested that *bš* 'is a word for any grain set aside for or specially prepared for beer-making,' and that it might mean 'malt'.³ Then I could only point to the records of a crude malting process prevalent in Egypt in Graeco-Roman times as evidence that malt was known.⁴ Prof. Leo Oppenheim has since published a study of brewing in ancient Mesopotamia, and shows therein that malt was used there from the Sumerian period onward, serving as a primary food as well as being used for brewing.⁵ Since malt, that is, sprouted barley, has been found in ancient Egyptian tombs, it seems probable that there was a similar development in Egypt.⁶

In regard to 'cutting the *bš*(?)' in O.K. tomb scenes, Prof. Keith Seele has pointed out to me, as I have already noted in my article referred to in the last paragraph, the analogous English expression, 'to mow hay', where hay is the end-product and not the name of the grass cut. The only N.K. reference to *bš* which gives any indication of its nature seems to me to make almost certain that this word is to be translated 'malt' in connexion with brewing. The tomb-chapel of Neferhotep (Th. Tomb no. 50) has the notation, 'Fourth month of Inundation, day 19. The day of moistening the *bš*, and spreading out the bed of Osiris NN.'⁷ This refers to the making of the sprouted seed-bed in the form of Osiris.⁸ Here again *bš* is the name of the final product and means, 'sprouted grain'. In the Moscow beer problems, *bš* is not a new element added, but only U.E. barley which has been 'sprouted' or 'malted' in preparation for brewing.⁹ The presence of malt, wheat, and dates on the Second Dynasty stela indicates that then, as in early Sumeria, malt was a primary food.

¹ See the list of such discoveries in Täckholm, *Flora of Egypt*, 1 (Cairo University, *Bulletin of the Faculty of Science*, no. 17), 142-6.

² Percival, 'Cereals of Ancient Egypt and Mesopotamia', *Nature*, 138 (1936), 271. The quotation continues, 'Egyptologists in particular have frequently fallen into the error of translating the Egyptian term for emmer by the word "spelt", and adding to it the name *Triticum spelta* instead of *T. dicoccum*.' It is unfortunate that Prof. Percival's observation has not received wider circulation.

³ *JNES* 9, 261 f., a review of Gardiner, *AEO*.

⁴ Lucas, *Ancient Egyptian Materials and Industries*³, 16 ff.

⁵ Hartman and Oppenheim, 'On Beer and Brewing Techniques in Ancient Mesopotamia' (Supplement, *JAOs*, no. 10, Dec. 1950), 13-15; see also *JAOs* 76, 201, n. 1.

⁶ Hans Helbaek, of the Nationalmuseet, Copenhagen, tells me he has examined malt from ancient Egyptian tombs which, if his memory serves him rightly, were O.K., probably Dyn. III, in date. He also calls my attention to Vivi Laurent-Täckholm, *Faraos Blomster*, København, 1952, 71, where is shown a picture of a cracked hollow scarab containing sprouted barley. The author does not refer to it as 'malt', but sees it as a symbol of resurrection, as in the 'bed of Osiris' illustrated on the opposite page.

⁷ *Wb.* 1, *Belegstellen*, p. 62, 418, 10; Schott, *Altägyptische Festdaten* (Mainz Ak., *Abh. d. Geistes- und Sozialwissenschaft. Kl.*, Jahrg. 1950, Nr. 10), p. 99. Schott has pointed out to me that the spell which follows is CT 1, 1.

⁸ Bonnet, *Reallexikon der ägyptischen Religionsgeschichte*, 391 ff., art. 'Kornmummien' and references there given.

⁹ I have considered and rejected the idea that *bš* is an old generic term for 'barley', as in the various O.K. scenes of granaries *bš*, U.E. barley and L.E. barley are all shown.

In problem 12 the given data are, '13 *hekat* of U.E. barley to be made into 18 *ds*-jugs of beer, *bšr mi bnr*, malt and dates alike (in amount)', with the notation, 'now, as for malt and dates alike, (it equals) $2\frac{1}{8}$ '. The answer is that the beer has a *psw* of 3, which would normally take 6 *hekat* of ingredients, not 13. It is conceivable that the addition of the dates to the brew was not counted in the *psw* calculation, and that 7 *hekat* of malt were exchanged for 6 of dates. But it is more likely, from our knowledge of Egyptian usage, that 3 *hekat* each of malt and dates were used, the total of 6 being used for the *psw* calculation, and that 10 *hekat* of malt was exchanged for 3 *hekat* of dates. Thus it took $3\frac{1}{3}$ *hekat* of malt to buy 1 *hekat* of dates.

The formula $\frac{2}{3} n bš n bnr$ is to be translated ' $\frac{2}{3}$ of the malt for the dates', and requires double the amount of barley normally used for beer, $1\frac{1}{2}$ *hekat* out of each 2 *hekat* of malt being exchanged for dates. Since the remaining amount of malt is only half that needed for beer of the *psw* named in each of the problems, the amount of dates purchased by $1\frac{1}{2}$ *hekat* of malt was $\frac{1}{2}$ *hekat*, and the dates were thus three times as expensive as the malt. Therefore the total amount of dates and malt were taken into account in determining the *psw*, as was assumed in connexion with problem 12. It may have been customary to make beer of equal quantities of malt and dates, since in the accounts from the Kahun papyri and P.Boulaq 18, in the three cases where the figures are preserved, the two occur in equal quantities.

As in the problems considered earlier, the cost of labour and processing was not considered in the value ratios, so in the problems with ' $\frac{2}{3}$ of the malt for the dates', the value of 1 *hekat* of dates was 3 *hekat* of U.E. barley.¹ The reason for the change of the formula in problem 12 to 'malt and dates alike, $2\frac{1}{8}$ ', was not to make the answer come out an integer. This could have been done with the usual ' $\frac{2}{3}$ ' formula by having the amount of grain used 12 *hekat* instead of 13. Rather it was because the cost of the dates was slightly higher.

There are, then, three ways in which prices are given in the Moscow papyrus. One is the straightforward value ratio, emmer being worth $2\frac{2}{3}$ times as much as U.E. barley. Another is the relative proportion of the amount of one ingredient needed to purchase another which is to be used in the product, $\frac{2}{3}$ of the barley-malt to buy dates, with the multiple of the normal amount of barley given as 2. Finally, only the multiple is given, $2\frac{1}{8}$, in problem 12. These latter usages in the beer problems make the calculations simpler than if the cost of the dates were given as a separate item. When these problems are thus explained, their difficulties disappear. Moreover, it is only by understanding that *bšr* means 'malt', a derivative of the U.E. barley, that the problems can be correctly worked out according to ancient Egyptian mathematical principles. Though they are differently stated, their nature is not much different from problems of present-day school arithmetic.

As an illustration of this, let us, in conclusion, state problem 16 in its Egyptian manner and then as it might appear today. The first is: 'Example of making a *ds*-jug of beer of 2 *psw*. According as was said to you, a *ds*-jug of beer of 2 *psw*, $\frac{2}{3}$ of the malt for

¹ As was pointed out in p. 60, n. 5, above, in Rhind 74 *wḏyt* is equated with U.E. barley in the same amount.

the dates, exchanged for emmer at the value of $2\frac{2}{3}$. The modern would be, 'A man sells a jug of beer, made of equal amounts of barley-malt and dates, for emmer. The beer has the strength of two jugs to the gallon of ingredients. The dates cost three times as much, and the emmer two and two-thirds times as much, as the barley-malt. How much emmer did he receive?'

Addendum—On the fractional *hekat* notations

Here must be entered a *caveat* in regard to the traditional translation of the *hekat* fractions. Prof. George R. Hughes has called my attention to an article by O. Neugebauer, 'Über den Scheffer und seines Teile', *ZÄS* 65, 44-48. In this the author attempts to show that the *hekat* fractions $\frac{1}{16}$ and $\frac{1}{32}$, as they appear in the hieratic, are not parts of the *wḏst*-eye notations but stand for 20 (*ro*) and 10 (*ro*). I do not find this quite convincing, especially in the light of his reasoning on p. 44. 'Endlich steht im "Moskauer mathematischen Papyrus" xxxviii, 4/5 problem 21: "bilde $\frac{1}{16}$ von 20 *hr ntt ir* > $\frac{1}{16}$ *pw*" und xxxviii, 3 ein analoger Satz über — und $\frac{1}{8}$. Wäre > selbstverständlich als $\frac{1}{16}$ zu lesen, so brauchte man nicht erst zu sagen "den $\frac{1}{16}$ ist $\frac{1}{16}$ "; wohl aber steckt darin eine für die Rechnung notwendige Bemerkung zur Division mit 16, wenn man es überlicherweise als 20 *ro* faßt.'

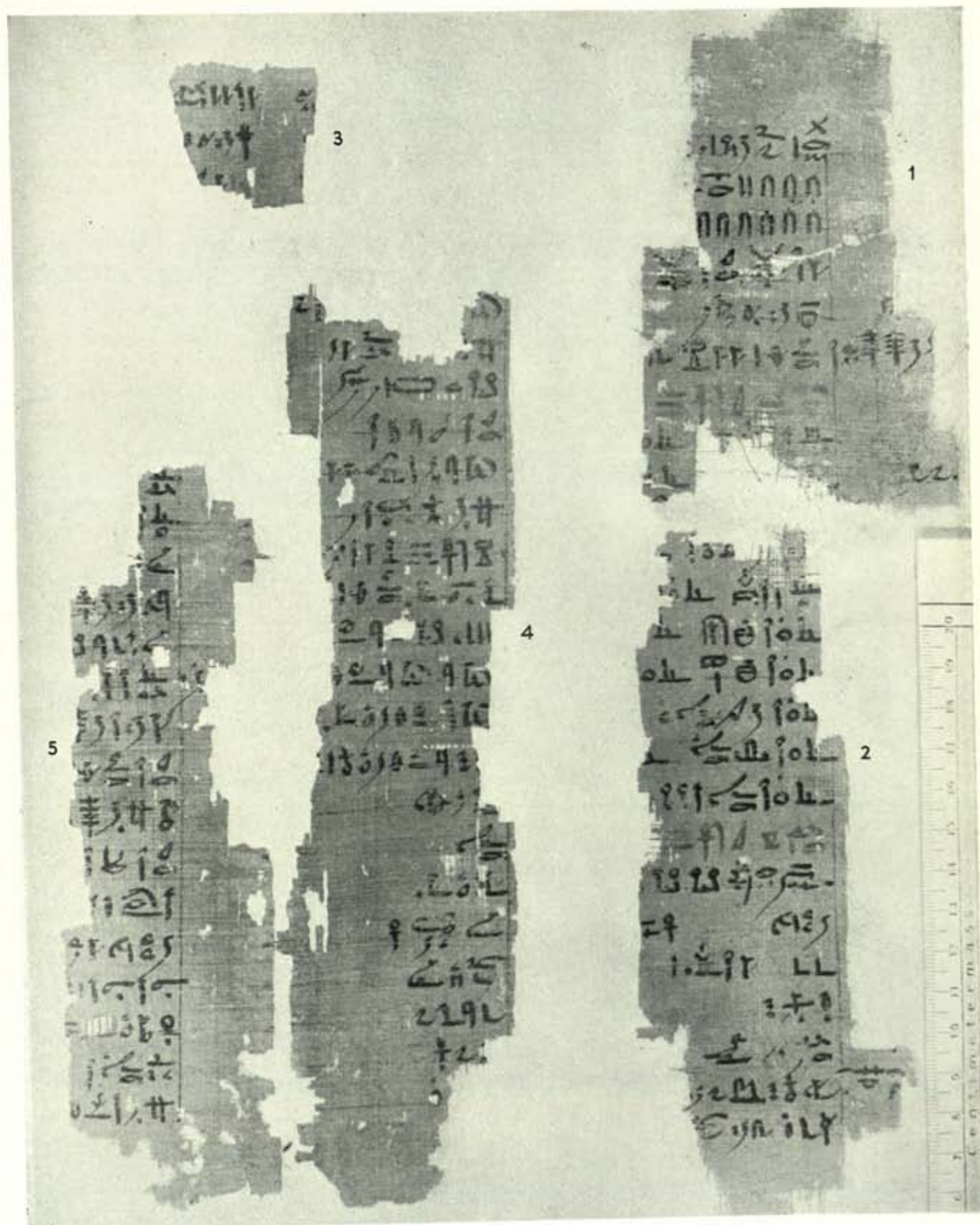
But certainly no Egyptologist believes that > is *per se* $\frac{1}{16}$ *hekat*; it is only the name of a measure that contains $\frac{1}{16}$ *hekat*, but since this name is unknown, we substitute the capacity instead. From English measures, using one archaic term and three from modern cookery-book usages, we can, on the basis of 1 *hekat* = *c.* 1 British Imperial gallon, give equivalent names for all the fractional measures. The series is: 1 gallon, 1 pottle = $\frac{1}{2}$, 1 quart = $\frac{1}{4}$, 1 pint = $\frac{1}{8}$, 1 cup(ful) = $\frac{1}{16}$, 1 gill = $\frac{1}{32}$, 1 glass = $\frac{1}{64}$, 1 tablespoon(ful) = $\frac{1}{320}$ = 1 *ro*, 1 teaspoon(ful) (U.S.) = $\frac{1}{3}$ *ro*. Putting at the beginning of this series 1 hogshead = 100 gallons (British molasses measure, 1749, *OED*) and 1 keg = 10 gallons (an arbitrary figure), we could write the answer to Moscow problem 20, $133\frac{1}{3}$ *hekat*, as 1 hogshead, 3 kegs, 3 gallons, 1 quart, 1 cup, 1 glass, 1 tablespoon, 2 teaspoons. Following the same method, what Moscow col. xxxviii, l. 5, says is not 'because $\frac{1}{16}$ is $\frac{1}{16}$ ', but 'because 1 cup is $\frac{1}{16}$ (gallon)'. And the answer to problem 16 would be '1 quart, 1 pint'.

GIESSEN UNIVERSITY LIBRARY PAPYRUS NO. 115

By R. O. FAULKNER

IN May 1957 Professors Parker and Caminos both wrote to me from Brown University, Providence, Rhode Island, calling my attention to a photograph published as Abb. 1 in the *Kurzberichte aus den Papyrussammlungen*, no. 2 (1956) of the Bibliothek der Justus Liebig-Hochschule (now happily restored to its former status as a University). These two scholars did so because Professor Neugebauer and they recognized in the photograph in question, which portrayed five fragments of a hieratic papyrus of late date bearing the number 115, a text which at first glance called to their minds the *Book of Hours* edited by me and recently published by the Griffith Institute, Ashmolean Museum, Oxford, and which I had previously summarily described in vol. 40 of this *Journal*. Further, Professor Neugebauer wrote to Dr. H. G. Gundel, Curator of the collections of papyri at Giessen, asking him to send me a copy of the *Kurzbericht* in question. Not only did Dr. Gundel do this, but he obtained for me the permission of the Director of the Library, Dr. J. Schawe, to edit this papyrus, and furthermore he has been untiring in supplying me with information as well as sending me excellent photographs. I express my heartfelt thanks both to my colleagues at Brown University and to the authorities of the Justus Liebig-University at Giessen for their kindness and co-operation.

Like the *Book of Hours* and the first part of P. Bremner-Rhind, this papyrus was written on one side only in tall narrow columns of one or sometimes two invocations to each line, see pl. XI. Since fragments 1 and 2 appear to be the upper and lower portions of a single column, then each column will have contained 25 or more lines, much as the similar texts named above; in that case the total height of the papyrus when intact will have been 31 or 32 cm., with blank margins of 3 and 2½ cm. at top and bottom respectively. The columns of text are aligned on vertical and horizontal guide-lines in black; an exception is the red vertical line tracing the left-hand edge of the almost totally lost right-hand column on fragment 3. The text appears to be of much the same nature as the *Book of Hours*, namely, invocations of Osiris and other deities which were to be recited on the appropriate occasion. Since the fragments now preserved to us contain sizeable portions of only three columns of text, it is comprehensible that the hour-rubrics, if any existed, have all been lost; but that this papyrus is not identical with the *Book of Hours* is shown by the interpolation at frag. 1, 7 and frag. 2, 8 of rubrics containing the ritual instruction *Make a libation of cold water*, such rubrics being entirely absent from the other text. Furthermore, no section of the Giessen text can be identified with any part of the *Book of Hours*, though in view of the losses the latter manuscript has sustained we cannot be quite certain that there was nowhere any measure of identity between the two. As regards fixing the date of the Giessen



fragments, we can rely only on palaeographic data, which are necessarily somewhat imprecise. The handwriting is larger than is usual in late hieratic manuscripts, but forms such as ⲁ with the body detached from the head (frag. 5, 11, Möller, *Hier. Pal.* III, no. 132); ⲁ without the tick on the back and with the base a straight line (frag. 4, 17;

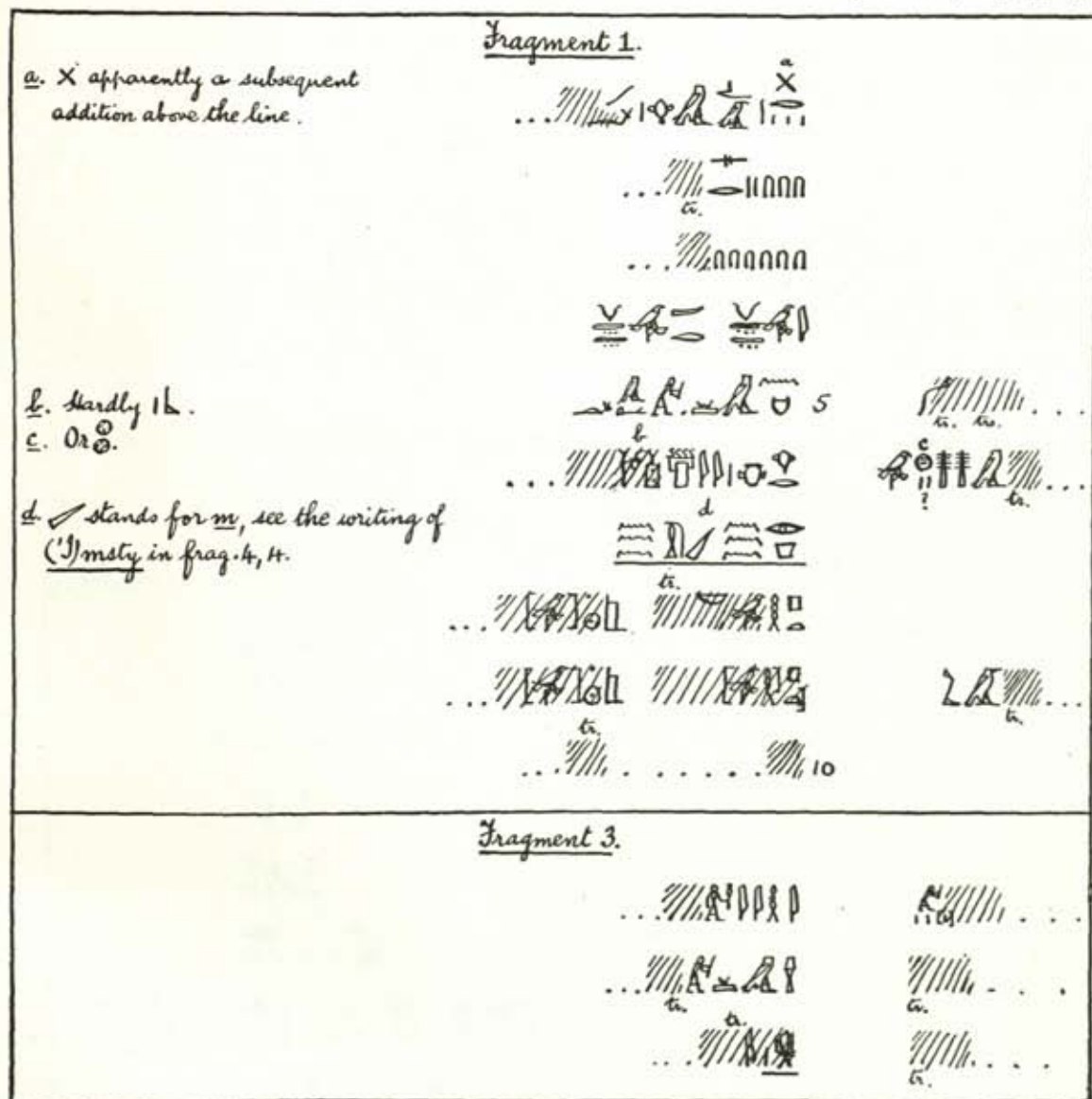


FIG. 1. P. bib. univ. Giessen No. 115, fragments 1 and 3.

Möller, no. 191); and ⲁ with a straight base (frag. 2, 14; Möller, no. 113) point to a date either late Ptolemaic or possibly early Roman. The writing of = as ⲁ does not appear to be recorded elsewhere. Of the provenance of the papyrus nothing is known, owing to the destruction of records during the war; it was acquired by the University of Giessen in 1912. The fragments were originally arranged and mounted under glass by Dr. Ibscher probably before the 1914 war, and it is probable that his arrangement

is that seen today, despite the fact that while still stored underground for protection from bombing the papyrus suffered from the infiltration of subsoil water into the cellars in 1945; fortunately in this case the subsequent damage was not serious.

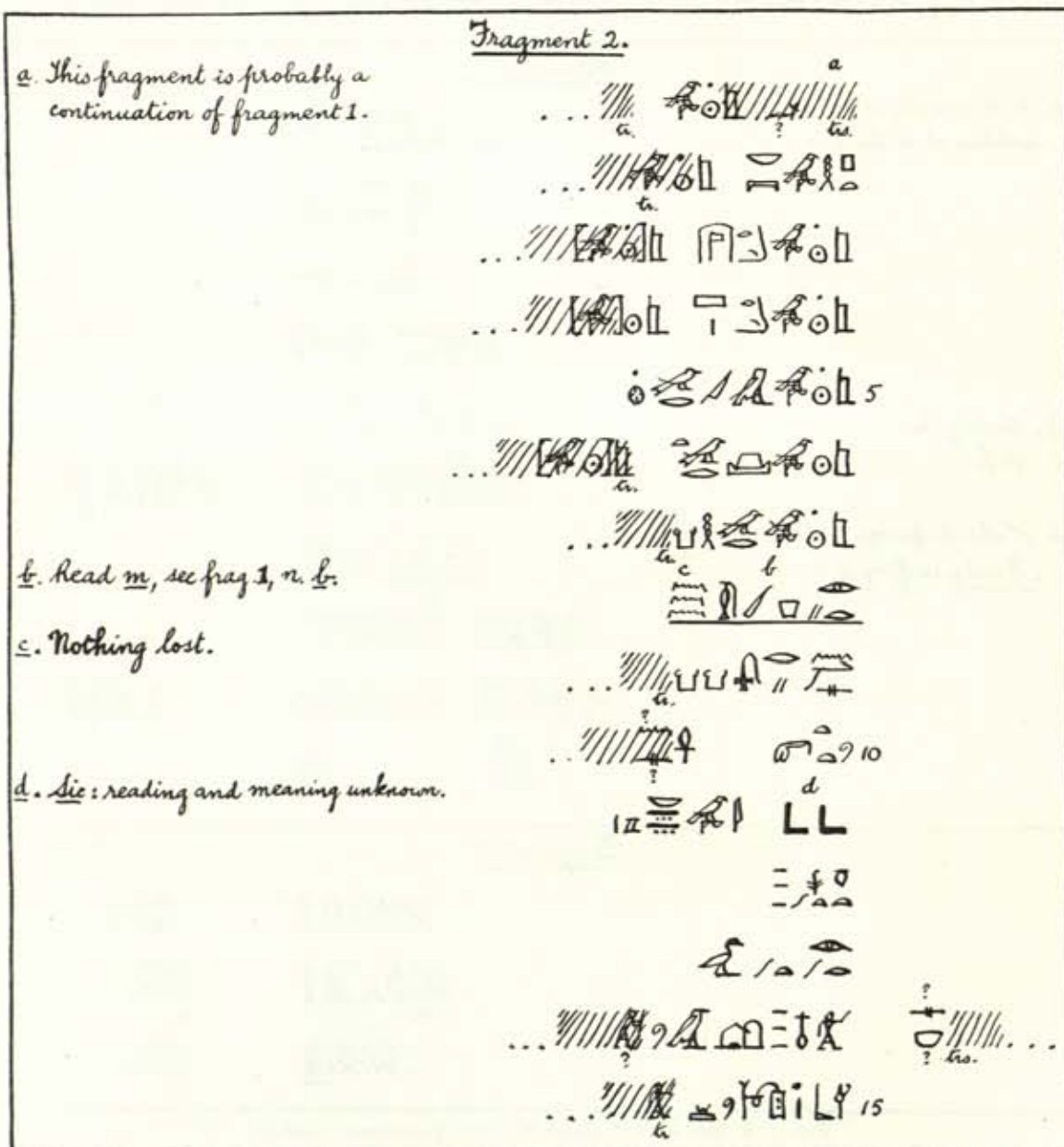


FIG. 2. P. bib. univ. Giessen No. 115, fragment 2.

A hieroglyphic transcription of the hieratic text, with rubrics underlined, appears in figs. 1-4, and translations of the five fragments follow.

Fragment 1 (fig. 1)

This fragment consists of ten lines from the top of a column with traces of a preceding

Fragment 4.

a. Only the most insignificant traces of the preceding column.

b-c. Read <I>msiy, one of the four children of Horus; the other three are named in ll. 5-7.

d. Or λ .

e. The space demands another l.

f. Reading not absolutely certain.

g. A bird-sign of indeterminate shape which somewhat resembles the more cursive forms of λ .

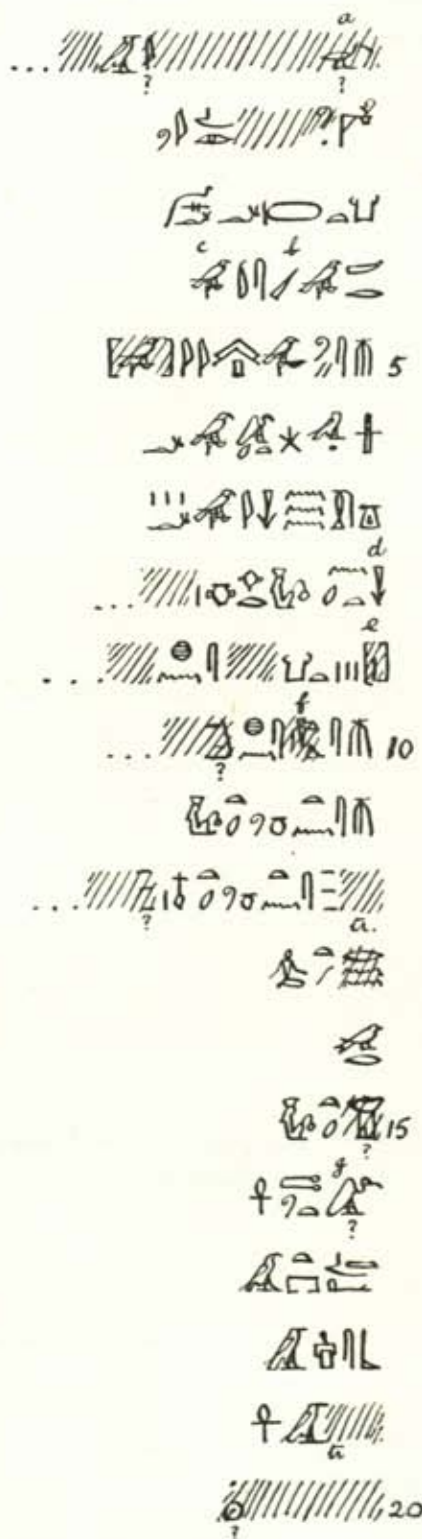


FIG. 3. P. bib. univ. Giessen No. 115, fragment 4.

column; of the latter there can be read only the words '... in Mendes (?)'—or 'Busiris'—at the end of what was probably l. 6.

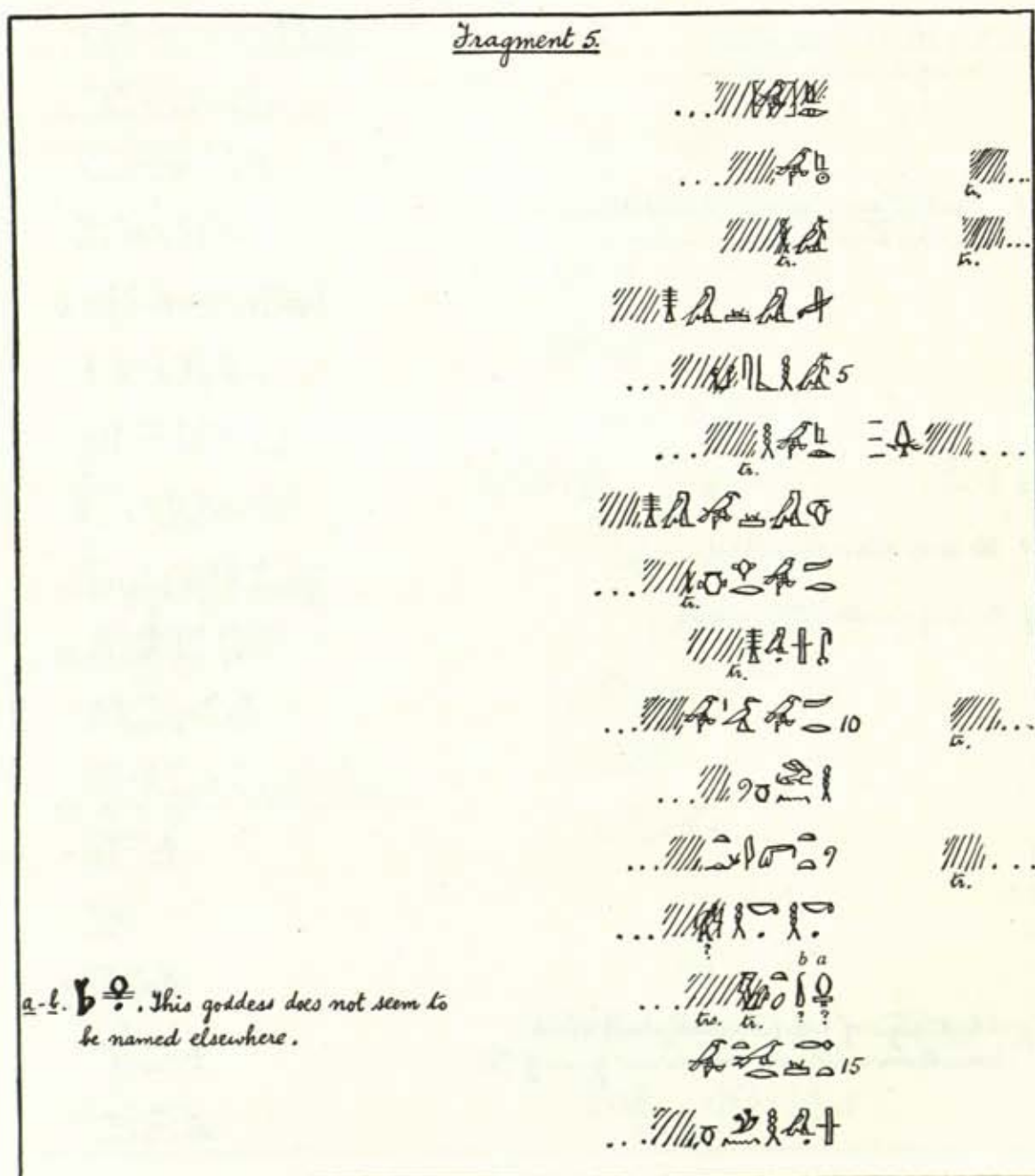


FIG. 4. P. bib. univ. Giessen No. 115, fragment 5.

Translation

The utterance of *M-m-hr-f* . . .
 Thirty-two . . .
 Sixty . . .

Thoth who judges the Two Lands. Horus who judges the Two Lands.
 5 He who is saved from him.
 He who dwells in the Portal, scribe of . . .
 MAKE A LIBATION OF COLD WATER.
 Ptaḥ, Lord of . . . Osiris . . .
 Ptaḥ . . . Osiris . . .
 10

Commentary

L. 1. This line seems to be a title ushering in a new series of recitations. *Mṣ-m-ḥrḥ* looks like an epithet of some deity, but its meaning is not obvious; a possible interpretation is 'he who sees with his face', but in that case we should expect *ḥrḥ* rather than *ḥrḥ*. The significance of the cross above the first word is obscure; possibly its purpose was to call attention to the fact that a new set of utterances was about to begin.

L. 2. The loss of the latter part of the line leaves the interpretation of *sr* . . . wholly uncertain.

L. 4. For *ḥrḥ* as a writing of the name of Thoth cf. Boylan, *Thoth*, 3. The fact that Thoth and Horus both receive the title of *wpt twy* must indicate that they were regarded here as but two aspects of a single deity.

L. 5. This sentence presumably refers to Horus, who was saved from the enmity of Seth.

L. 6. Perhaps Thoth is referred to here, though the first epithet is not known to Boylan; he is the being most likely to be described as 'scribe'.

L. 7. Ritual directions such as this are entirely absent from the *Book of Hours*.

Fragment 2 (fig. 2)

The allusion to Ptaḥ in l. 2 is an indication that this fragment continues frag. 1. Of the preceding column only the very end of the last line but one is preserved.

Translation

. . . Osiris . . .
 Ptaḥ, Lord of heaven. Osiris . . .
 Osiris pre-eminent in the God's Booth. Osiris . . .
 Osiris pre-eminent in She(?). Osiris . . .
 5 Osiris in *Ds-wr*. . . .
 Osiris of the Great Granary. Osiris . . .
 Osiris great of magic. . . .
 MAKE A LIBATION OF COLD WATER.
 The flame of . . .
 10 The Begetter. He who lives (?) . . .
 . . . Thoth, Lord of the Two Lands.
 The Southern Nūt.
 Offer a goose(?).
 O Fair Ones who fight . . .
 15 Reversion (of offerings) in writing (?) . . .

Commentary

L. 3. *Sh ntr* 'the God's Booth' is the term for the embalming booth, which is usually presided over by Anubis, but Osiris is associated with it, as occupant rather than as presiding deity, also in *Book of Hours*, 11, 12.

L. 4. Despite the absence of the town-determinative, *Š* is more likely to refer to the town of She (Crocodylopolis) than to a mythical lake. On She cf. *Wilbour Papyrus*, IV (Index), 88.

L. 5. This locality appears to be unknown.

L. 9. I have no suggestion as to the meaning of the damaged word *kḳ*; the plant of that name can hardly have been meant.

L. 12. As the text stands the translation 'the Southern Nūt' seems unavoidable, but the appearance of the text, with its lack of any specific determinatives, is suspicious, and hints that corruption has crept in. A possibility that suggests itself is a mis-writing of *Nnt*, the personification of the sky below the earth.

L. 13. This looks like a ritual direction to be read *ir t(rp)* 'offer a goose', but in that case we should expect it to be written in red rather than black.

L. 15. Lit. 'written reversion'; the determinative of *wdb* is quite indecisive, but no other sense of the stem appears to fit the allusion to writing.

Fragment 3 (fig. 1)

This contains only portions of three lines from a left-hand column and traces of a right-hand column. Its connexion with the other fragments is not known.

Translation

The Sistrum-player . . .

The Mighty One . . .

PROTECTION . . .

Fragment 4 (fig. 3)

This fragment bears twenty lines from the bottom of a column, of which nine are complete. Probably not more than five or six lines are missing from the top of the column, but whether frag. 3 belongs to the lost portion is quite uncertain.

Translation

.....

The standard . . . the lion(?).

He who devised his own name.

Horus and Imsety.

5 The child of Hapy.

He who is in Duamutef.

The throne of Kebehsenuf.

Senet who dwells in . . .

The four souls(?) of Sekhen . . .

10 The child of Sekhen.

The child of Setenut.

. . . of Setenut, beautiful . . .
 Renēnet.
 The Great One.
 15 Nephthys(?).
 Mother of the living(?).
 The righteous ones.
 The Protector(?).

 20

Commentary

L. 2. The absence of a determinative renders the rendering of *mꜣw* as 'lion' open to question, but 'he who sees' does not seem very likely. For the omission of determinatives see also ll. 17. 18 below.

Ll. 5-7. These curious expressions do not seem to occur elsewhere.

L. 8. Instead of 'Senet' it would be possible to read also 'Shenet'.

Ll. 9-10. A deity Sekheny is recorded in *Wb.* iv, 254, 13.

L. 11. For the reading *Stnwt* rather than *Tnwt* see the next line. This goddess is probably identical with $\text{𓎡} \text{𓏏} \text{𓏏}$, *Hymns to the Crown*, 18, 5. The name may mean 'the crowned one', from *stni* 'to crown', recorded by *Wb.* iv, 358, 13 from *Ombos*, i, 118. 153, cf. also *stnw* 'Crown of Upper Egypt', *ZAS* 49, 15. 34. A similar epithet is used of Osiris in *Book of Hours*, 11, 26, and of Sokar in P. Bremner-Rhind, 18, 2.

L. 13. The goddess of nursing.

L. 16. The reading and sense of this line are both highly doubtful, and again corruption is possible. Here the whole group preceding 𓎡 is taken to be a writing of *mwt* 'mother', but with little confidence.

L. 17. The sense is hardly in doubt, despite the absence of determinatives and the unaccountable insertion of \square into the writing of the word.

L. 18. The omission of the determinative obscures the sense of the word, but a derivation from *bs* 'protect', *Wb.* i, 475, 8, seems probable. An allusion to the minor deity Bes is much less likely.

Fragment 5 (fig. 4)

This fragment contains portions of the lower sixteen lines of a column of which only one, l. 15, is complete. There are also some mostly indecipherable traces of the ends of lines belonging to a previous column.

Translation

Osiris . . .
 Osiris . . .

 Leader in Busiris(?).
 5 Nehebkau . . .
 Osiris . . .
 Khnum in Busiris(?).

- Horus who dwells in . . .
 The Sceptre which is in Busiris(?).
 10 Horus son of [Isis?]
 Youth . . .
 He who begat [his?] father.
 The Old One . . .
 Khetemtit(?) . . .
 15 The Great and Mighty One.
 He who is in . . .

Commentary

L. 4. The word of which \mathfrak{H} alone is preserved (so also ll. 7. 9) is surely either *Ddt* 'Mendes' or *Ddw* 'Busiris', in this context more probably the latter.

L. 5. The reading *Nhb-kw* seems certain, despite the intrusive *s*, which has doubtless been inadvertently taken over from *hbs* 'clothe'.

L. 6. The sign after 'Osiris' transcribed as \mathfrak{K} could also read \mathfrak{I} , in which case we would have to restore $\mathfrak{I}[\mathfrak{A}\mathfrak{B}\mathfrak{C}]$. The trace after \mathfrak{K} does not suit *hwnw* 'youth', for which see l. 11 below.

L. 10. The restoration 'Isis' as the end of the line is almost certain.

L. 11. For *hwnw* 'youth' as an epithet of Osiris cf. P. Bremner-Rhind, 1, 10. 14; *Lamentations of Isis and Nephthys*, 2, 3.

L. 13. For *khkh* 'old' cf. *Wb.* v, 138, 10 ff.; the word is doubtless connected etymologically with *khkht* 'hacking' of a cough, cf. Blackman in *JEA* 13, 188.

L. 14. The reading is doubtful and the name apparently unknown.

L. 15. This line might apply to either a male or a female deity, since the feminine ending at so late a period has no significance.

L. 16. This incomplete place-name cannot be identified.

THE INTERPRETATION OF THE HORUS-MYTH OF EDFU¹

By J. GWYN GRIFFITHS

VERY divergent explanations have so far been offered of this version of the conflict of Horus and Seth. On the whole, the tendency has been to reject the original view that the legend reflects a cult war in favour of the view that it incorporates an early historical tradition.²

Newberry³ believed that the Edfu story is in essentials a record of the Seth-rebellion of Peryebesen in the Second Dynasty. In Naville, *Mythe d'Horus*, pl. 11, he saw a representation of King Djoser's vizier, Imhotep, 'reading from a scroll as though he were actually reading a record of the war written in the lines of inscription in front of him'; in front of Imhotep is a figure who is cutting up a hippopotamus, and Newberry takes the animal to represent the 'country of Set', which Imhotep directs to be cut up. But it is doubtful whether this is the Imhotep of Djoser's time. The inscription⁴ above refers to him as 'the chief lector, scribe of the god's book'; the reference to the original Imhotep is present only in so far as he had by this time become the half-deified type of sacred scribe.⁵ The hippopotamus is doubtless a Sethian animal here; but hardly a symbol of Seth's country.

Again, Newberry compares the statement from the Edfu text, that the rebellion arose when the Horus-king was with his army in Nubia, with the record on a fragment of a stela of Khasekhem, commemorating that king's conquest of Nubia. Although the captives depicted in *Hierakonpolis*, II, pl. 58 are probably meant to be Nubians, there is no inscription describing the conquest, and it is clear that a similar comparison might be made with episodes from the lives of a number of Egyptian kings.

The Edfu rebellion is dated in the 363rd year of Harakhti. Newberry takes this as an era dating: 'It gives the number of years from the establishment of the monarchy by the Horus-king Menes to the time of the outbreak of the Set rebellion recorded in the text.' Meyer's restoration of a part of the Annals Stone is followed, whereby 375 years are counted from the accession of Menes to the beginning of the reign of Khasekhemui—a difference of twelve years from the era date at Edfu. The correlation seems good enough as it stands; but even the small difference involved could be accounted for, argues Newberry, if it were presumed that the ancient annalists recorded the reign of Khasekhemui only from the time he united the whole country. This is altogether too ingenious.

¹ I am indebted to Professor H. W. Fairman for criticisms and suggestions; also to the late Professor A. M. Blackman, with whom I read the texts.

² See especially Kees, *Kultlegende und Urgeschichte* (Nachr. Göttingen, 1930).

³ *Ancient Egypt*, 1922, 40-46.

⁴ Chassinat, *Edfou*, VI, 87, 9. Naville takes it to refer to the architect of part or whole of the Edfu temple.

⁵ Cf. Sethe, *Imhotep*, 17, and Kees, *op. cit.* 345.

Brugsch¹ would base the legend on the polemics of the local priestly societies, each with its special doctrine and festival calendar. An historical element is introduced by Maspero² in the theory that the followers of Horus of Edfu, the *Msntyw*, translated 'Smiths', are connected with an African people who became dominant through the discovery of the use of iron weapons; otherwise, he follows Brugsch in claiming the struggle to be a theological one in which the scenes of conflict are the nomes where Seth has a sanctuary and partisans.

Sethe,³ on the other hand, suggests vaguely that historical reminiscences may be conserved in the legend of Mesen, 'Harpoon-City', the name frequently applied to Edfu in the legend. Both Meyer⁴ and Junker⁵ claim that the myth reflects the original conflict between Horus and Seth which they place in predynastic times. H. R. Hall⁶ thinks that the myth is a late working-up of historical reminiscences of the arrival of the Upper Egyptians from Nubia and the south. H. W. Fairman,⁷ who completed, with A. M. Blackman, a much-needed study of the myth in the way of translation and commentary, regards Newberry's opinion as 'attractive and plausible', but reserves his own opinion for a future statement.

Kees⁸ marks out as the two chief features of the legend:

- (1) The driving out of Seth over the north-east boundary near Sile, the god being connected with the hereditary Asiatic enemy.
- (2) The conflict with the cults of the crocodile and the hippopotamus, hated in the falcon-cities, which forms the kernel of the harpoon-myth and is skilfully interwoven with the destruction of all the Sethian cult-places in Egypt.

He dwells on the early identification of Seth with the foreign land or the desert,⁹ but argues against connecting the legend generally with predynastic or early dynastic events. He points out that in the early texts Horus and Seth are described as sharing Egypt between them; now Horus is given the whole¹⁰ and Seth is driven out. This may be the result of his becoming the state-god of the Hyksos, and his subsequent association with the arch-enemy of Egypt, Semitic Asia. His temporary return to power in the Ramesside period was followed by a general persecution, which would have been very strong under the bigoted orthodoxy of the Ethiopic rule.

¹ *Abh. Göttingen*, 14 (1868-9), 177.

² 'Études de mythologie et d'archéologie égyptiennes', in *Bibl. égyptol.*, 11, 313 ff. Sethe's view that the *Msntyw* are 'harpooners', i.e. hunters of the hippopotamus, is now generally accepted, e.g. Kees, *op. cit.* 349, and Fairman in *JEA* 21, 29, n. 2.

³ *Urgeschichte*, §§ 155-62. In § 161 he states: 'Man könnte denken, daß wirklich in Edfu, am damaligen Süden des Landes, der letzte Schlag in dem Kampf der Unterägypter des Reiches von Damanhur gegen die Oberägypter des Reiches von Ombos gefallen sei . . .'

⁴ E. Meyer, *Gesch. Alt.*, 3rd edn. 1, 2, §§ 181, 199.

⁵ H. Junker, *Onurislegende*, 20.

⁶ H. R. Hall, *Ancient History of the Near East*, 94.

⁷ H. W. Fairman, 'The Myth of Horus at Edfu, I', in *JEA* 21, 28, n. 2. His statement that 'Kees . . . claims that these legends have no historical value' is an exaggeration of that scholar's position. Blackman and Fairman in *JEA* 28, 32-33 give prominence to Sethe's view.

⁸ *Op. cit.* 355.

⁹ It is doubtful whether Seth was the god of foreign countries as early as the Old Kingdom. See Gardiner and Gunn, *JEA* 5, 44, n. 2.

¹⁰ This is already the case with Geb's second verdict in the *Denkmal memphitischer Theologie*.

Kees must be granted to have demonstrated at least that if the legend is based on history, its connexions are other than prehistoric. Seth, he says, frequently appears in this legend as the representative of Lower Egypt; which is never the case in the early literature. The persecution of the cults of the hippopotamus and crocodile, which is so important an element, must be placed at least in the New Kingdom. Sâve-Söderbergh¹ has pointed to earlier instances of hippopotamus-hunting being represented or alluded to; but a Sethian meaning is not clearly attested before the New Kingdom. The Ptolemaic texts of Edfu and Denderah show that the Horus-cities led in this persecution, and the Edfu legend includes among the cities which supported such an attitude the cities of Osiris and Min.

To these facts emphasized by Kees may be added the manifest difference in the conception of the kingship. In the Pyramid Texts the king is sometimes represented as an incarnation of Horus and Seth, and this can be construed as a clue to the historical meaning of the political unity achieved after the predynastic conflict. There is no such fusion of the two gods in the Edfu legend. The king is Rē-Ḥarakhti, and Horus of Behdet is his chief guardian god. Seth, on the other hand, is completely degraded in a manner which would not have been possible in any text originating from the Old Kingdom. It may be argued, of course, that all these differences are accretions contributed by a late theology, and that the matter which conserves the early conflict is only a thin substratum or a bare outline. But the difficulty of defining this substratum is great, since it is not only the theology that is different, but the topography of the quarrel and the main details of the action. At least three Horuses are prominent in the Edfu account—Horus of Behdet, Horus the son of Isis, and Horus the Elder. Helio-*polis* and *Pr-rh*,² are no longer important in the topography of the conflict, and Geḥesti and Nedeyet are not mentioned. The mutilation of the eye of Horus does not figure in the action; generally there is only a mass attack on the crocodiles and the hippopotami, which is often followed by a vengeful sacrificial meal,³ paralleled in the Pyramid Texts only by the sacrificial eating of the bull.

The interpretation offered by Kees is that parts of the myth, especially the Legend of the Winged Disk, reflect a cult feud rather than a political conflict.⁴ At the same time he sees in the whole myth the impress of two great historical experiences, the expulsion of the Hyksos and, more vividly, the expulsion of the Persians. A reference to the latter experience is found by him in the use of the word *Mdy*,⁵ which he translates 'Mede'

¹ *On Egyptian Representations of Hippopotamus Hunting as a Religious Motive* (Horae Soederblomianae, Uppsala, 1953). On p. 17 he cites an example which is probably prehistoric: it is on a schist palette (fig. 8) now in the Egyptian Museum at Stockholm. He wisely refrains from suggesting that the hippopotamus at this stage represents Seth. E. J. Baumgartel, *The Cultures of Prehistoric Egypt*, 30, 33-35, 65, 84, is equally cautious; but not so Wainwright, *The Sky-Religion in Egypt*, 11.

² According to Chassinat, *Edfou*, VI, 121, 13, Rē moors his barque there, but no fighting takes place.

³ E.g. Horus of Behdet, according to Chassinat, *op. cit.* VI, 116, 8 ff., brought 142 enemies before Rē: 'He slew them with his knife and gave their inner parts to those who were in his following and gave their flesh to every god and goddess who was in this barque of Rē on the bank of Hebenu.' Cf. *ibid.* 127; and II, 65; I, 68; VI, 119, 7 ff. a personal combat between Seth and Horus of Behdet is described, but without the ancient details.

⁴ *Op. cit.* 348.

⁵ See Chassinat, *op. cit.* VI, 214, 12 and 215, 2.

and which Sethe had connected with the Coptic ⲙⲁⲩⲟⲓ 'soldier'. The Late Egyptian idiom of the section about the 'Red Hippopotamus' may be an argument for Kees's interpretation of that section; it suggests, at least, in company with other differences, that the various parts of the myth may diverge greatly in origin and meaning. The particular reference to the Persians is, however, doubtful. It is disconcerting, for one thing, that the term *Mdy* is applied to Horus and not to Seth,¹ even if the appellation is scornful. If Seth truly represents the Persian invader, he would not be ridiculing Horus by calling him a 'Mede'.² Sethe, it is true, connects the Coptic ⲙⲁⲩⲟⲓ with the Egyptian *Mdy*, suggesting that the meaning 'soldier' developed from the meaning 'Mede'.³ The present passage, however, strongly suggests that this development has already taken place, and that *Mdy* here means 'soldier'. 'Rē said to Thoth, What is this they are speaking of, Horus and Seth? And Thoth answered, Seth said to Horus, Let us call the *Mdyw* with the names of the foreign countries. Horus said to Seth, A challenge to the name of the Egyptians from Seth.'⁴ Now the foreign-land determinative supports the view that *Mdy* means 'Mede', at least originally. But in the passage quoted, this meaning yields very poor sense. The *Mdyw* are here clearly equated with the Egyptians; further, if they did mean 'Medes', it would be no insult to give them foreign names. Applied as the term is to Horus and his followers, it probably denotes armed Egyptian soldiers.

As this is the main point in Kees's 'Persian' interpretation, it cannot be said that his position is well founded. On the other hand, the view here put forward as to the meaning of *Mdy* in this context would involve giving a still later date, perhaps, to this section of the legend.

The Winged Disk

There are certain facts about the legend of 'The Winged Disk' which suggest that it may be historical. The struggle against Seth is led by the 'King of Upper and Lower Egypt, Rē-Ḥarakhti';⁵ Horus of Behdet and Horus son of Isis are only his assistants.⁶ The opening, with its description of the king's return from Nubia, does not pretend to be mythological. Action in Nubia is also mentioned later on, as a part of the general campaign against Seth and his confederates.⁷ The campaign begins near Edfu, and results in the driving of the enemies into the sea.

It must be confessed that a number of places mentioned in the description of the drive northwards seem to owe their prominence to cult propaganda and conflict. The

¹ Chassinat, *op. cit.* VI, 214, 12: 'Seth said, Come, *Mdy*! It was said as a challenge.'

² *Wb.* II, 177 (21) knows *Mdy*, written with the Seth-animal as a determinative, as an epithet of Seth. It does not apparently record the present word.

³ Sethe, *Spuren der Perserherrschaft in der späteren ägyptischen Sprache* (Nachr. Göttingen, 1916), 124 ff. It was formerly thought that the Coptic ⲙⲁⲩⲟⲓ was derived from the Egyptian *Mdry*, which was identified by Schäfer with the word *Mdy* as used in the Nastesen inscription. See H. Schäfer, *Die aethiopische Königsinschrift des Berliner Museums*, 41-42; and for *Mdry* see F. Ll. Griffith, *Rylands Papyri*, 319, and Gardiner, *JEA* 3, 105; 5, 47, n. 2.

⁴ Chassinat, *Edfou*, VI, 214, 12-215, 3.

⁵ *Ibid.* 109, 9.

⁶ *Ibid.* 110, 2 ff.: 'And Horus of Behdet was in the barque of Rē.' For Horus son of Isis, see VI, 120, 4 ff.

⁷ *Ibid.* 128, 7 ff. Seth is not mentioned in connexion with the first reference to Nubia.

nome of Denderah¹ had a feather over the crocodile as its sign, and this was interpreted, at least in late times, as the triumph of Osiris over Seth.² Hebenu,³ where a fight is staged, is known for the falcon over the gazelle in its nome-sign, explained with reference to Horus and Seth.⁴ The latter has well-attested associations with Meret⁵ in the Oxyrhynchite nome.⁶ *Pr-rhwy*, where a great slaughter is said to take place,⁷ is placed near Meret. With the exception of *Trrw* and *Hnt-i:bt*, which figure in the final phases of the struggle, the other places mentioned, such as Naref⁸ and *St-i:b-i*,⁹ are associated with Osiris and Horus.

A prominent feature which is in favour of viewing the conflict as a cult feud is the theological etymologizing with which the story is constantly punctuated; for example, the struggle near Meret contains the episode:

Then Horus of Behdet waged war with the enemy for a long time. He hurled his spear at him, and cast him on the ground¹⁰ in this town; and it is called 'The House of the Two Rivals' (*Pr-rhwy*) to this day.¹¹

The same impression is given by the naming of festivals and their times, such as the 'festival of rowing' in *Pr-rhwy*;¹² and by the ritual emphasis of such a comment as this on the victory of Horus:

He has acted according to the book of repelling the hippopotamus.¹³

One must agree with Kees, therefore, in following Brugsch's belief that the conflict represented in this section of the legend is in the main a cult conflict. There is no trace here of the alleged association of Seth with the Persians; but there are some reasons for claiming that the expulsion of the Hyksos has left a subsidiary impression on the legend. One is the prominence of the nome of Sile in the account. A great slaughter and sacrifice are said to take place in *Trrw*,¹⁴ also called *Hnt-i:bt*;¹⁵ and this culminates in the complete expulsion of the enemies from Egypt.¹⁶ *Hnt-i:bt*, 'the beginning of the East',

¹ Ibid. 115, 6: 'He saw them to the north-east of the nome of Denderah.' The writing is not, as far as I know, used of Denderah itself, although Fairman, *JEA* 21, 30, takes it in that way.

² Mariette, *Denderah*, III, 78. The sacred marriage between Horus of Edfu and Hathôr of Denderah ensured local hostility to Seth and the crocodile. Cf. Kees, *Horus und Seth*, II, 43; Sethe, *Urgeschichte*, § 49.

³ Chassinat, op. cit. VI, 116, 5 ff.

⁴ See Junker, *Omurislegende*, 37-38; Kees, op. cit. II, 23; Sethe, op. cit. § 61. The sign appears thus in Chassinat, op. cit. VI, 70, 3.

⁵ Chassinat, op. cit. VI, 118, 2 ff.

⁶ See Kees, op. cit. II, 44-45. The text itself is suggestive of a cult quarrel: 'They reached the water of the nome of Meret, and that confederacy of Seth which is in this town' (Chassinat, op. cit. VI, 118, 2-3).

⁷ Chassinat, op. cit. VI, 118, 6; 119, 8; 121, 6. It is probably to be read *Pr-rhwy* and not *Pr-rhwy* as Gau-thier, *Dict. géog.* II, 107, and Fairman, *JEA* 21, 31, would read it. The occasional double | may be due to the conventional writing of the dual: see *Wb.* II, 441 and 442. The name may occur on a predynastic palette, see Petrie, *Ceremonial Slate Palettes*, G 19 and p. 14; but this is very doubtful.

⁸ Chassinat, op. cit. VI, 123, 3, and Fairman, *JEA* 21, 33.

⁹ Chassinat, op. cit. VI, 123, 1 ff., and Fairman, loc. cit. n. 4.

¹⁰ Accepting Fairman's plausible emendation, loc. cit. 31, n. 6.

¹¹ Chassinat, op. cit. VI, 119, 7 ff.

¹² Ibid. VI, 121, 8-9. Cf. VI, 123, 1 ff. and VI, 126, 3 ff.

¹³ Ibid. VI, 114, 2. Cf. Schott, *Urk.* VI, 61: 'The Ritual of Repelling the Evil One.'

¹⁴ Chassinat, op. cit. VI, 127, 7 ff.

¹⁵ Ibid. 127, 14.

¹⁶ Ibid. 128, 2: 'Rê said to Horus of Behdet, Let us sail to the sea, that we may drive the enemies as crocodiles and hippopotami from Egypt.'

was the name of the 14th Lower Egyptian nome, and its capital *Tirw*, Sile near El-Kantāra, was the place where the caravan-route left Egypt for Palestine and which was naturally the point of influx for invaders and the point for an Egyptian frontier fortress against Asia. In the same nome was Tanis, which is known to have been a centre of government of the Hyksos.¹ Avaris was probably on or near the site of Tanis.²

The Expulsion of Seth from Egypt

It is hard to avoid the conclusion that in the references to the triumphant ejection of Seth from the country there are embodied reminiscences of the expulsion of the Hyksos. No such ejection occurs in the early accounts of the quarrel, and its presence in the Edfu account can hardly be explained on the lines of cult propaganda, since certain centres of the cult of Seth must have remained unaffected by the persecution inculcated in the myth.³ The ejection of Seth via *Tirw* may be said to be the culminating point of the legend of 'The Winged Disk'. In the more avowedly ritualistic section on 'The Triumph of Horus' there is a suggestion of the same idea, but with a difference: here the expulsion is seen in triumphant retrospect. Horus of Behdet is 'the lion who presides in *Hnt-ib*, who has driven Seth from Lower Egypt, goodly defender of the Two Lands and of the Banks, the protector who protects Egypt'.⁴ He is 'the goodly watchman in the Two Lands and the Banks, who protects the cities, who defends the nomes, the falcon, great in might in Pe and Mesen, the lion who presides in *Tirw*'.⁵ Politically more precise, in its reference to a subjugation of Lower Egypt and the subsequent unification of the whole country—corresponding broadly to the achievement of the Upper Egyptian régime which drove out the Hyksos—is the statement that Horus is 'the lion who presides in *Tirw*, falcon great in might, Lord of Upper and Lower Egypt, defender who defends Egypt against the Northerners, wall of copper

¹ A monument of Ramesses II at Tanis is explained thus; cf. Sethe, *Urgeschichte*, 187. *Tirw* was formerly taken to be Tanis; see Gardiner's survey in *JEA* 5, 244, n. 6, and Budge, *The Gods of the Egyptians*, 1, 100. *Tirw* is placed near Ismailia by Steindorff, *Die ägyptischen Gaue*, 864 ff.; cf. Erman, in *ZÄS* 43, 73. For its location at Sile see Kees, *Horus und Seth*, 11, 72; Sethe, *Urgeschichte*, § 78.

² The geography of Tanis, Avaris, and other places in the North-east Delta is exhaustively discussed by Gardiner in *JEA* 3, 99 ff.; 5, 127 ff., 242 ff.; 10, 94 ff.; and 19, 122 ff., where he comes eventually to the conclusion that Avaris, Pi-ra'messe, and Dja'net were successive names of the same place, Tanis or Šan-el-Hagar. Pi-Ra'messe and Tanis are mentioned separately in the Onomasticon of Amenope, and Gardiner in *AEO* 11, 173*–5* seems a little more hesitant about their identification. Kees in *Das alte Ägypten*, 109 expresses the view that Pi-Ra'messe was in Tanis and that Avaris was closely adjacent to Tanis; cf. Montet, *Géographie de l'Égypte ancienne*, 1, 193 ff.

³ Cf. Kees, *Kultlegende u. Urgeschichte*, 361, where it is stated that there is no clear evidence that the outlawing of crocodile and hippopotamus, which the Horus-myth demands, had gained general recognition. The Suchos-cult especially was secure in the Upper Thebaid, in spite of the proximity of cities which supported the cult of Horus.

⁴ Chassinat, *Edfou*, vi, 65, 2–3.

⁵ Ibid. 71, 10–11. Cf. ibid. 72, 7–8: 'Horus of Behdet, great god, lord of heaven, protector who protects the cities and the nomes, whose arms are stretched around Upper and Lower Egypt, his Mesen-city being their leader'; and ibid. 84, 1–3: 'Horus of Behdet . . . lord of the *šm'-s*-crown, ruler of the *mḥ-s*-crown, King of the Kings of Upper and Lower Egypt, excellent ruler, ruler of rulers. I take hold of the crook and the flail as the lord of this land, I seize the Two Lands with the Double Crown, I overthrow the enemy of my father Osiris as King of Upper and Lower Egypt for ever and ever.' This exclusive kingship of Horus is very different from the reconciliatory double kingship portrayed sometimes in the Pyramid Texts.

round his Upper Egyptian Mesen, watchman of his Lower Egyptian Mesen'.¹ Sile is now a frontier fort against Asia, as Edfu is in the south against Nubia. Its position as the Lower Egyptian counterpart of Edfu is explained in relation to a victory over Seth.²

By a process of syncretism the Hyksos made the Seth-religion the religion of the state.³ Seth was worshipped from early times in the North-east Delta, so that the invaders may have only renewed the cult in that region and joined it with one of their own.⁴ An example of the identification of Seth and Baal occurs in the Edfu myth,⁵ but this could derive, of course, from a much later period. More significant for the interpretation of 'The Winged Disk' and 'The Triumph of Horus' is the fact that the struggle between the Hyksos and the Egyptians who expelled them seems to have been regarded partly as a struggle between Rē and Seth. The story of the quarrel of Apophis and Sekenenrē illustrates this. It is stressed that Seth or Sutekh is the god of Apophis. The name of Sekenenrē and the mention of Rē-Ḥarakhti⁶ and Amenrē⁷ indicate that Rē in some form is regarded as the rival deity. Hatshepsut, describing her reconstruction after the havoc wrought by the Hyksos, complains about the Asiatics who had lived in Avaris that they 'ruled without Rē'.⁸ It is indeed clear that, in spite of the prominence of Horus of Behdet and the other Horuses in the Edfu myth, it is Rē who is its theological mainspring. Horus of Behdet is himself but 'the image of Rē in Upper Egypt'.⁹ Rē-Ḥarakhti describes him as 'the son of Rē, exalted one who came forth from me'.¹⁰ According to a text edited by Schott,¹¹ the Horus-falcon is 'the living *ba* of Rē'. In the same way Thoth, in this system of theology, as Blackman pointed out to me, is 'the heart of Rē'.¹² In the narrative of 'The Winged Disk', which has a more historical

¹ Ibid. 75, 5-6.

² See Kees, *Horus und Seth*, II, 72; Sethe, *Urgeschichte*, § 148; Kees, *Kultlegende u. Urgeschichte*, 358. It should be noted that Sile does not figure in the other sections of the myth. The fight between Horus son of Isis and Seth takes place to the east of Edfu, see Chassinat, op. cit. VI, 135, 2. 6. 11; the story of the 'Red Hippopotamus' has its centre in Edfu and Elephantine; and in the story of the 'Red Donkey' the fight occurs in the 10th Upper Egyptian nome (op. cit. VI, 220, 5-6), Seth being connected, as in early times, with Shashetep as well (ibid. 221, 2).

³ Cf. P. Sallier I, I, 2-3, trans. Gardiner and Gunn in *JEA* 5, 40: 'Then King Apophis took Setekh to himself as lord, and did not serve any god which was in the entire land except Setekh.'

⁴ See Junker in *ZÄS* 75, 77 ff. on the cult at Sethroë, and Černý on the still earlier cult of the god in the Delta, *Ann. Serv.* 44, 295 ff.

⁵ Chassinat, op. cit. VI, 71, 12.

⁶ Gardiner, *Late-Egyptian Stories*, 86, 1-2.

⁷ Ibid. 87, 2. For a new interpretation of the story see Sève-Söderbergh, *On Egyptian Representations of Hippopotamus Hunting etc.*, 43 ff.

⁸ *Urk.* IV, 390, 9. Gunn suggests that the meaning implies their refusal to act by means of a divine oracle. See Gardiner in *JEA* 32, 55. Sève-Söderbergh in *JEA* 37, 64 describes both this statement and that in P. Sallier I as 'a propagandist exaggeration'. Mayani, *Les Hyksos et le monde de la Bible*, 120, translates 'qui régnaient sans connaître Rē', but 'connaître', which Breasted suggested, should be deleted.

⁹ Chassinat, op. cit. VI, 113, 5; cf. VI, 113, 2-3: 'Then Rē-Ḥarakhti said to Horus of Behdet, This is my image in Upper Egypt.' See also Fairman's note, *JEA* 21, 29, n. 3.

¹⁰ Chassinat, op. cit. VI, 111, 3.

¹¹ *Urk.* VI, 75, 9-12. In the context it is used syncretistically of the ram of Mendes. For the falcon as the *ba* of Rē, both at Edfu and at Philae, see Schott's note ad loc.; Junker, *WZKM* 26, 42 ff.; Kees, *Kultlegende u. Urgeschichte*, 353.

¹² Chassinat, op. cit. VI, 92, 13: 'Thoth . . . the heart of Rē.' Cf. ibid. VII, 322, 7 and Chassinat, *Dendara*, I, 28, 12; I, 64, 11-12; II, 170, 10; III, 9, 1-2; III, 19, 17; III, 52, 9; III, 67, 11-12. I am indebted to Blackman for the references to Chassinat, *Dendara*.

appearance than any other section of the myth, Rē-Harakhti, as we have already observed, is the leader of the campaign against Seth.

Against this must be noted the remarkable fact that many of the Hyksos kings had their names compounded with Rē.¹ It is the opinion of Labib² that the fight between Apophis and Sekenenrē is, in religion, the fight between Sutekh and Amūn. Unlike 'The Book of the Victory over Seth',³ the myth of Edfu contains no reference to the return of Seth after his first expulsion. In the former text, Seth is said to be driven out of Egypt 'to the land of the Asiatics'.⁴ Later, however, Rē-Harakhti is entreated to remember that he had commanded 'to give Egypt to Horus, the desert (?) to Seth';⁵ in the meantime the enemy has returned: 'Behold, Seth, the wretched one, is come upon his way, he has returned in order to seize with his hand; he has planned to seize violently (?), as he formerly destroyed places (var. houses). . . .'⁶ His vile deeds in attacking Egypt are then described, and Rē eventually renews his banishment from Egypt: 'Seth shall not stay in Egypt. For it shall be forbidden him (to stay there).'⁷ Spells follow which are intended for the protection of Egypt against the invader. All this may be referred with some reason to the later Asiatic invasions, or the threat of them. So far Kees⁸ may be right, and there is some evidence supporting a detailed application of the idea. But it is to be noted that the absence of any mention of the second coming of Seth in the Edfu myth, taken in conjunction with other facts, is an argument for seeing in the ejection of Seth in that myth a reminiscence of the expulsion of the Hyksos.

In the case of the Assyrians there was indeed no forcible expulsion. Psammetichus I was favoured by the Assyrians, and it was only owing to the difficulties of his foreign masters that he paved the way for Egypt's comparative freedom in the Twenty-sixth Dynasty.⁹ Concerning the end of the Persian domination we know more since the publication by Kraeling of the important Aramaic papyri in the Brooklyn Museum. Kraeling¹⁰ has been able to show, on the basis of this new evidence, that Artaxerxes II (404-361 B.C.), and not Darius II, was the last Persian king to rule over Egypt; it was the rebellion of his brother Cyrus, aided by the Greek expedition of Xenophon, that weakened his position and enabled the revolt led by Amyrtaeus, of Manetho's Twenty-eighth Dynasty, to be fully successful. It appears, however, that the national revival in Egypt at that time was not comparable to the attitude prevalent in the Twenty-sixth Dynasty.¹¹ The renaissance of the Saïte period, which probably witnessed a religious

¹ See Pahor C. Labib, *Die Herrschaft der Hyksos in Ägypten und ihr Sturz*, 13. 23. 24. Junker, *ZÄS* 75, 81 says that we must not press the Hatshepsut inscription when it says that the Hyksos did not know Rē (sic). He points out that their kings generally have the *sr-R* title. But it is very probable that the names, and of course the titles, were adopted by them and did not originally belong to them: see Engberg, *The Hyksos Reconsidered*, 46.

² Op. cit. 36.

³ Edited by Schott in *Urk.* vi.

⁴ Ibid. 13, 6.

⁵ Ibid. 17, 17.

⁶ Ibid. 17-19.

⁷ Schott, *ibid.* 26 translates *nn is wq tw n-f* 'denn es ist ihm nicht befohlen'. Gunn pointed out in lectures at Oxford that *n wq* is the regular Egyptian equivalent for 'to forbid'. On p. 24 one should therefore translate similarly: 'He knows not the fear of Thy Majesty; he approaches Egypt when thou knowest not, although it has been forbidden to him.'

⁸ Kees, *Kultlegende u. Urgeschichte*, 358.

⁹ Breasted, *History of Egypt*, 565 ff.

¹⁰ E. G. Kraeling, *The Brooklyn Museum Aramaic Papyri*, 31-32. I am indebted to Fairman for calling my attention to this work.

¹¹ Breasted, *op. cit.* 595.

revival involving the persecution of Sethian cults, was not paralleled afterwards.¹ A work referring to Seth in this spirit and referring to an Asiatic invasion after the expulsion of the Hyksos would be more likely for these reasons to emanate from the Saïte period. It is just possible that a section of 'The Book of Victory over Seth' refers in some detail to the invasion of the Assyrian Esarhaddon, who captured Memphis.² The misdeeds of Seth are said in this section to concern Memphis especially:

He has inflicted misery in Tjenent [a sanctuary at Memphis], he has devised rebellion in Memphis. Lo, he enters into the holy place of Memphis.³ (Schott, *Urk.* vi, 19, 10 ff.)

No allusion of this kind occurs in the Edfu myth.

That 'The Winged Disk' and 'The Triumph of Horus' are to be connected rather with the expulsion of the Hyksos is suggested by the Nubian associations of Horus of Behdet. Nubia figures twice in 'The Winged Disk', not without the suggestion that it is part of Rē-Ḥarakhti's kingdom. In 'The Triumph of Horus' it is said of Horus:⁴

Behold, thou art a Nubian in Khenfet. Thou sittest in thy temple, and Rē has given thee his kingdom that thou mayest overthrow the hippopotamus. (Chassinat, *Edfou*, vi, 69, 9-10.)

Kees⁵ quoted a text from Edfu which describes Horus as 'Horus of Edfu, the sacred falcon who came out of Weten⁶ to unite himself with Edfu as the lord of the throne'. He quotes too⁷ from a building-block in Cairo: 'Erecting a temple for Horus of Nubia in the nome of *Wtst-Hr.*' Further, in 'The Winged Disk', as we have already noted, the text begins with a mention of the king's return from Nubia; and a campaign there against Seth and his followers is described afterwards.

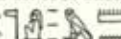

In spite of the paucity of the records dealing with the expulsion of the Hyksos, Nubia figures in the actions of both Kamosē and Amosis. The former relates his position in the well-known Carnarvon Tablet No. I:⁸

Let me understand what this strength of mine is for! (One) prince is in Avaris, another in Kush, and (here) I sit associated with an Asiatic and a Nubian.

The stela of Kamosē, which Labib Habachi discovered in 1954 in front of the second pylon of Karnak, reveals that the Hyksos king Apophis (*ꜥꜣ-wsr-Rꜥ*) sought to effect an alliance with the King of Nubia against Kamosē and that the latter's soldiers intercepted a dispatch which Apophis hoped to send to Nubia.⁹ But it was Amosis, the first

¹ Spiegelberg, tr. Blackman, *The Credibility of Herodotus' Account of Egypt*, 7; Gardiner and Gunn in *JEA* 5, 45. In the Ramesside era Seth had new power as a state god, but the Libyan Dynasty which followed the Ramesside kings did not persecute Seth. It seems that Seth was not merely tolerated by them at a distance, as Kees suggests, *Kultlegende u. Urgeschichte*, 357, but was held in honour. Cf. his role in the Dakhlah Stela which derives from the Twenty-second Dynasty, see Gardiner in *JEA* 19, 19 ff.

² Breasted, op. cit. 555.

³ Schott: 'das Serapeum'. In Schott, *Urk.* vi, 19, 20 ff. it is said of Seth: 'He has devised conflict, he has given forth a roaring in the presence of the gods in Menset (). *Wb.* II, 88 gives  as a place in or near Heliopolis.

⁴ Or of his harpoon?

⁵ *Kultlegende u. Urgeschichte*, 354.

⁶ According to Kees an African land in the south-east.

⁷ Kees, loc. cit.

⁸ Cf. Wilson, *The Culture of Ancient Egypt*, 164 and Gardiner in *JEA* 3, 99.

⁹ See Labib Habachi in *Ann. Serv.* 53, 195-202 and in *Les Grandes Découvertes archéologiques de 1954* (Cairo, 1955), 52-58; M. Hammad in *Chron. d'Ég.* 30, 198-208; and cf. Siegfried Horn in *Bibl. Orient.* 14, 216-17.

king of the Eighteenth Dynasty, who, after driving the Hyksos from the Delta, won victories in Lower Nubia and recaptured the territory up to Buhen.¹ If there is, then, a correlation here with the story of the Edfu myth, the exploits of Amosis will provide it. Certainly the double activity of the two accounts, set in the North-east Delta and in Nubia, is a striking resemblance.

It is true that the Karnak stela of Kamosē makes it clear, as Säve-Söderbergh² has pointed out, that Kamosē also attacked both Lower Egypt and Nubia. The relevant allusion may be translated thus:

Do you behold³ what is being done to Egypt⁴ in opposition to me? A⁵ ruler who is within it, Kamosē-ken, given life, is pressing me from my domains. I had not attacked him in the fashion of all he did against you. He relegates these two lands to torment, my land and yours, since he has devastated them. (lines 20-22.)

There is doubt about the reference of some of the tenses in this stela, but not about the past tense of 'he has devastated them'. Kamosē clearly attacked Nubia first; and afterwards the Hyksos positions in Lower Egypt. Still, the final victory was achieved by Amosis, and his exploits were therefore more likely to be remembered.

The thesis which finds in the Horus-myth of Edfu the impress of the Hyksos invasion and of their eventual ejection must clearly rest to some extent on the conception of the Hyksos as hated foreign invaders. Säve-Söderbergh⁶ has argued against such a conception having prevailed from the beginning among the Egyptians. He suggests⁷ that the term *hkꜣw hꜣswt* 'gives us the impression that the Hyksos were only a little group of foreign dynasts rather than a numerous people with a special civilization'. The interpretation of the phrase seems still an open question; but there is evidence to suggest that Asiatic infiltration on a larger scale had taken place previously. Hayes⁸ makes a just comment to this effect in considering the significance of a list of forty-five Asiatic persons attached to the household of an Upper Egyptian official. It might be argued that his generalization ('If, as seems likely, similar groups of these outlanders were to be found in well-to-do households throughout the whole of Egypt, then the Asiatic inhabitants of the country at this period must have been many times more numerous than has previously been supposed') is too confident. But his instance is the more cogent for being located in Upper Egypt. Asiatic influence would be more easily felt, it stands to reason, in for example the Eastern Delta.

¹ Säve-Söderbergh, *Ägypten und Nubien*, 143; and in *JEA* 37, 71.

² 'The Nubian Kingdom of the Second Intermediate Period', in *Kush*, 4, 54-61, especially p. 57.

³ Säve-Söderbergh, *Kush*, 4, 57: 'Do(n't) you see . . .'. For the sense of *nonne*, however, a negative would be expected in Egyptian, see Gardiner, *Eg. Grammar*³, § 491, 3 and § 492.

Labib Habachi and S.-S.: 'what Egypt has done against me'; Hammad: 'what Egypt has done to me'. Such a bold personification of 'Egypt' as an agent seems unlikely in spite of the earlier sentence 'they have abandoned Egypt, their mistress'. In the same stela we find the phrases 'within Egypt' (18), 'in Egypt' (23) and 'the towns of Egypt', and the first of these phrases refers to the damage done by Egyptians who were helping the Hyksos. The Hyksos king was of course in possession of a part of the country, so that Kamosē can hardly be equated with Egypt here. Further, a present tense is more consonant with the *hr tm* that follows. On the other hand, the expression *irt nꜣ nbt rꜣk* (21-22) favours the other rendering.

⁵ Rather than 'the': Kamosē was not the only one. Indeed, *hkꜣ* suits the Hyksos ruler as well in a special way.

⁶ *JEA* 37, 53-71 ('The Hyksos Rule in Egypt').

⁷ *JEA* 37, 56.

⁸ *A Papyrus of the Late Middle Kingdom*, 148-9.

Säve-Söderbergh¹ states further that 'according to Manetho's version it also seems as if the Hyksos rule only meant a change of political leaders in Egypt, and not a mass-invasion of a numerically important foreign ethnic element'. This explanation is hard to understand. Manetho suggests just the opposite, as the following excerpt from Waddell's translation (p. 79, quoted also by Säve-Söderbergh) shows:

... and unexpectedly, from the regions of the East, invaders of obscure race marched in confidence of victory against our land. By main force they easily seized it without striking a blow; and having overpowered the rulers of the land, they ... treated all the natives with a cruel hostility.

Here the phrase τὸ γένος ἄσημοι is admittedly vague,² but the eastern provenance of the invaders is emphasized. Their overwhelming number is suggested by κατὰ κράτος; and their foreign origin is again stressed by the allusion to their treatment of the natives. There is, of course, no necessary contradiction between a sudden military and political take-over and a previous gradual infusion of Asiatic elements into the population.

Discussing the text of the Carnarvon tablet, Säve-Söderbergh³ rightly stresses the mild interpretation given by the king's grandees to the Hyksos domination: 'The Hyksos are not regarded only as cruel and oppressive godless barbarians—the usual picture in the later sources—it is possible to make a deal with them and to live in peace with them.' But one is justified also in noting the trenchant attitude of Kamosē himself. Säve-Söderbergh well translates one sentence thus (p. 68): 'I will grapple with him and rip open his belly, for my desire is to deliver Egypt and to smite the Asiatics.' This is the nationalism of the native leader facing alien domination; and it follows that a feeling of hostile hatred towards the Hyksos is attested from one part of a contemporary source.

Säve-Söderbergh would agree, presumably, that the attitude of hostility was in any case very evident in later times, so that it could have coloured—if the hypothesis is in other ways acceptable—allusions to the Hyksos as Sethian enemies in the Edfu myth.

Conclusion

The Horus-myth of Edfu, in so far as it reflects a historical-political rather than a cult feud, probably mirrors the ejection of the Hyksos. There are no clear allusions in it to the expulsion of either the Assyrians or the Persians. Another late text, 'The Book of Victory over Seth', may, on the other hand, contain a reference to the overthrow of Memphis by the Assyrian Esarhaddon.

The campaigns against the Hyksos, which Kees finds reflected in the myth (as well as those against the Assyrians and Persians), can perhaps be related in some detail to certain episodes in 'The Winged Disk' and in 'The Triumph of Horus', since these episodes invite correlation with the Egyptian victories in the North-east Delta and in Nubia. At the same time much of 'The Winged Disk' concerns struggles between different cults and most of 'The Triumph of Horus' has a ritual purport.

¹ JEA 37, 56.

² Engberg, *The Hyksos Reconsidered*, 4, translates 'a people of ignoble origin'. This derogatory sense of ἄσημος is well attested and is on the whole preferable, since the invaders' origin is clearly not imagined as unknown.

³ JEA 37, 69.

ANOTHER WITNESS-COPY DOCUMENT FROM THE FAYYŪM

By H. S. SMITH

In Memoriam S. R. K. Glanville

STEPHEN GLANVILLE died with tragic suddenness on Thursday, April 26, 1956. On his desk were photographs of some unpublished demotic papyri in the British Museum, which he had brought back to Cambridge the day before. I wished as his pupil and friend to publish these, and Mr. I. E. S. Edwards, Keeper of the Department of Egyptian Antiquities at the British Museum, very kindly obtained permission from the Trustees of the Museum for me to do so, and has given me every encouragement. By a happy chance the main document is concerned with the same house and family as the first demotic contract ever published by Glanville.¹

These papyri were deposited in the British Museum at some time during the nineteenth century. Unfortunately there is no record of their acquisition, but they were rolled up in a single bundle and may have been found together. They were very much broken, but with the skilled technical assistance of Mr. Baker of the Department of Egyptian Antiquities, I have succeeded in placing all but the minutest inscribed fragments. The following documents have emerged: (i) B.M. 10750, a deed of cession in respect of a house in Philadelphia in the Fayyūm dated in year 9 of Ptolemy IV Philopator, which forms the subject of this article; (ii) B.M. 10774 A and B, substantial portions of complementary deeds of sale and cession in respect of half a house, almost certainly also in Philadelphia, which may be dated to year 20 of Ptolemy V Epiphanes; (iii) a portion of a Greek tax receipt in respect of tax on a house in Philadelphia, dated in year 14 of a Ptolemy, almost certainly Philopator. It is hoped that a publication of the last two items by Mr. T. C. Skeat and myself will appear in Vol. 45 of this *Journal*. Neither of them appears to have any direct connexion with the transaction of B.M. 10750.

P. B.M. 10750

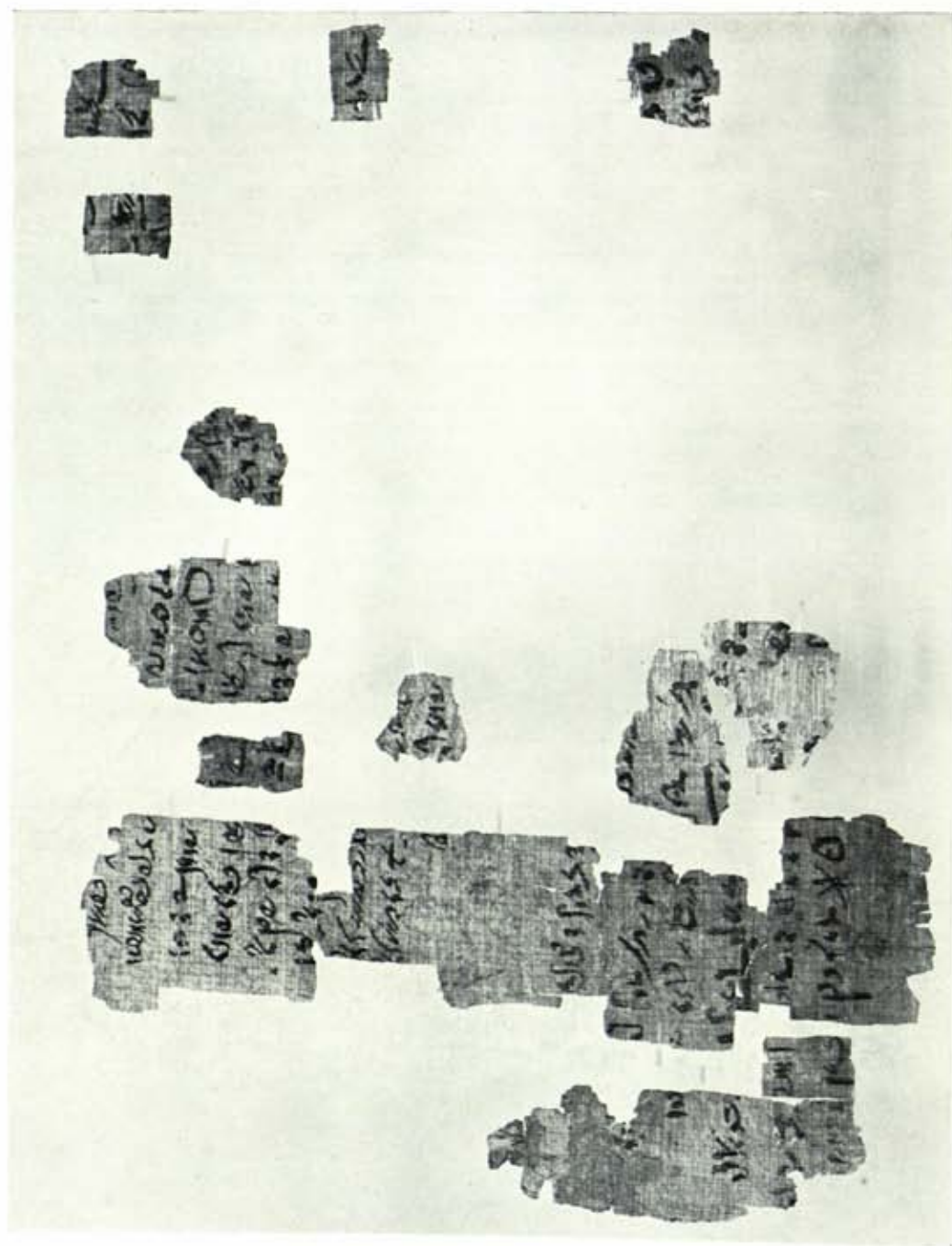
Two complete copies (AB, Pl. XII) and substantial portions of three damaged copies (CDE, Pl. XIII) of this deed are preserved. The alignment of the papyrus fibres proves beyond doubt that all five copies originally formed part of a single roll. Copy A alone bears the scribe's signature; it is therefore the original deed and stood at the beginning of the roll. Copy E stood at the end, since a portion of the vertical inner edge of the roll, now obscured by the binding, is preserved in the margin to the left of it.² The last six lines of copy D are above copy E; the first six lines of D with copy C above

¹ B.M. 10616, in *Griffith Studies*, pp. 152-60 and pls. 15-20.

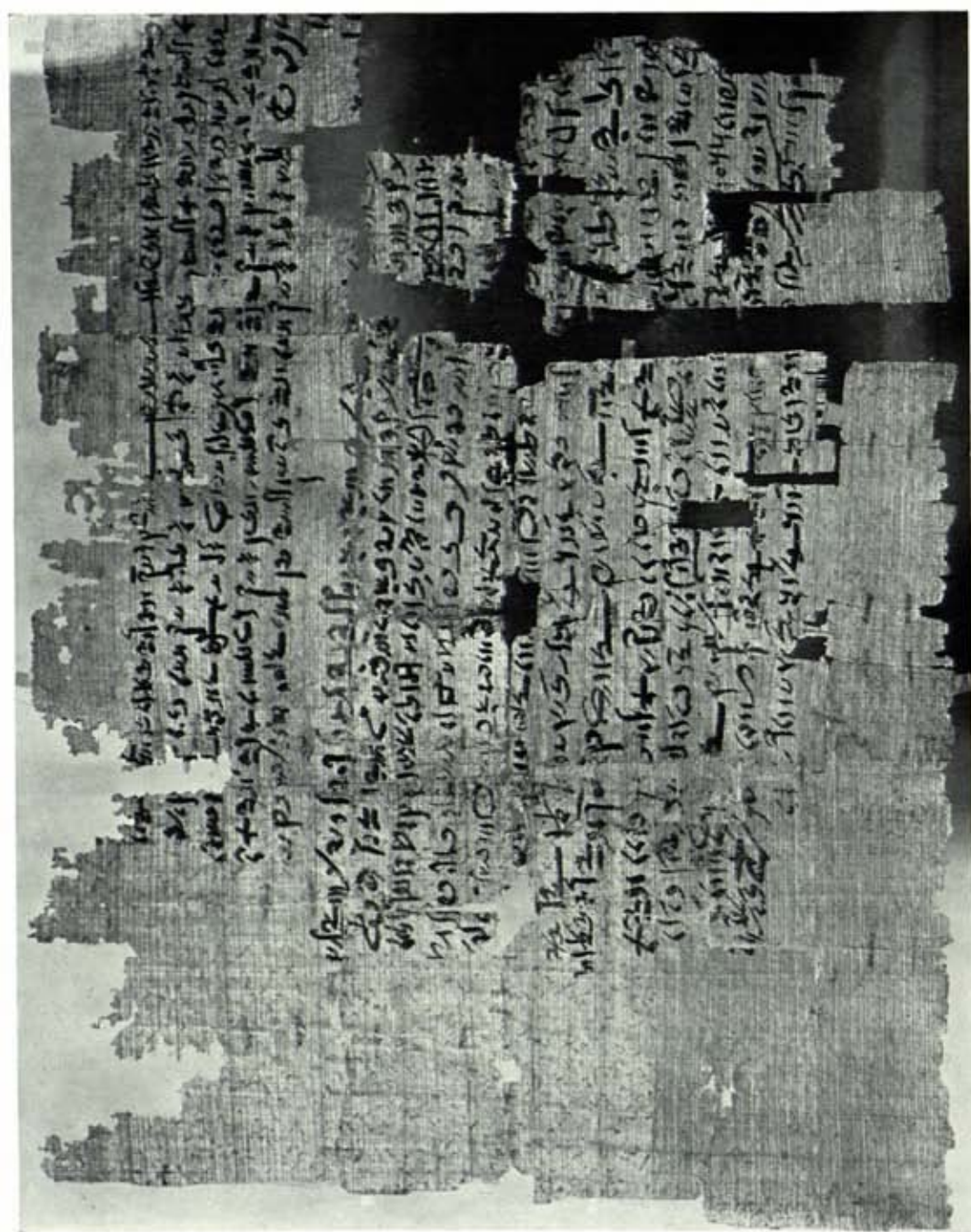
² Higher up this margin there is a horizontal pen-stroke, broken off to the left, which can hardly be accidental. It cannot be the beginning of a further copy since nothing appears vertically above or below it. It was probably part of a brief scribal note, the nature of which I cannot suggest.

Handwritten text in a cursive script, likely a manuscript. The text is written in a dark ink on a light-colored background. The script is dense and fills most of the page. There are some large, stylized characters that appear to be initials or section markers. The text is arranged in several lines, with some lines being longer than others. The overall appearance is that of an old, handwritten document.

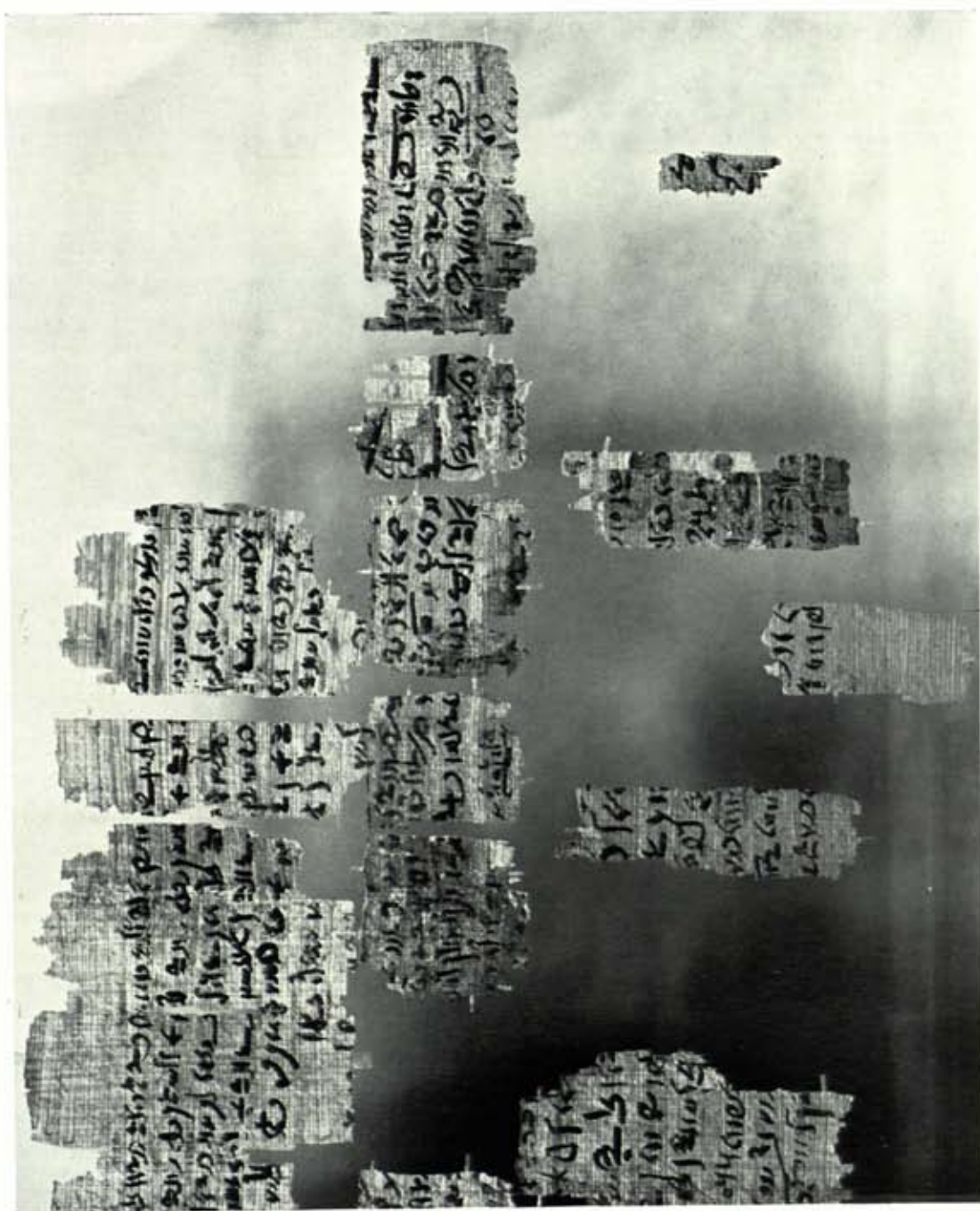
Handwritten manuscript page from the Voynich manuscript, featuring two columns of text written in the characteristic Voynich script. The text is densely packed and includes several large, stylized initial letters or symbols at the beginning of sections. The handwriting is fluid and consistent throughout the page.



P.dem. Brit. Mus. 10750: C and D 1-6



P.dem. Brit. Mus. 10750: D 7-12 and E (left half)



P. dem. Brit. Mus. 10750: D 7-12 and E (right half)

must therefore have stood immediately to the right of them. The possibility that further copies, of which no single fragment has survived, stood between B and C may be virtually dismissed; of twelve surviving witness-copy documents of the Ptolemaic period known to me, ten at least have five copies including the original.¹ The present widths of the portions of the roll containing AB and DE are approximately 27 in. and 29 in. respectively; allowing for lost margins, a reasonable estimate for the original length of the roll is 7 ft. 6 in. or just over. The maximum height is now 12 in. and was probably never much more.

The papyrus is light brown in colour, rather coarse, but well made. The recto is stained with large patches, which Mr. Baker tells me are glue, probably ancient. The rectos of B.M. 10774 A and B are similarly stained, and Mr. Baker suggested that at some time a scribe may have gummed the papyri face to face in order to use the relatively blank versos. At all events he did not do so, for no later writing is to be seen on the versos. The only two scraps of writing preserved on the verso of B.M. 10750 are on the back of two fragments forming the top right-hand corner of copy C on the recto, and are not reproduced in the plates. They clearly contain the ends of names forming part of the witness-list, which thus stood near the middle of the verso of the roll.

B.M. 10750 is dated twenty-seven years later than any demotic contract with witness copies written out in full hitherto known. Twenty-four of these documents are extant to my knowledge;² three of Saite date from Hibeh (Rylands I, II, and VI); three from Edfu (Cairo 50151+50152+50158, probably mid-fourth century, Hauswaldt 10 of 264 B.C., Hauswaldt 2 of 240 B.C.); two from Philadelphia in the Fayyūm (B.M. 10616 of 244 B.C., B.M. 10750 of 213 B.C.); and sixteen from Thebes, all dating between 335 and 267 B.C. The evidence is clearly still insufficient to support firm conclusions about the history of this type of document, but it is notable that our last example from Thebes, whence we have a considerable bulk of evidence, antedates both documents from the Fayyūm.

B.M. 10750 is a *sh wy*, conventionally translated 'deed of cession'. The *sh wy* was a formal acknowledgement by the first party that ownership of a property and all rights over it had legally passed from him to the second party, together with an undertaking not to dispute those rights; it therefore always contained a reference to the legal instrument by which the second party had acquired ownership, which was usually a deed of sale,³ *sh etbe ht*, dated the same day. In B.M. 10750 the woman Soptnoufri and her two

¹ P. B.M. 10026, 10616: Hauswaldt 2, 10: Louvre 2424: Philadelphia VII, X, XIII: Rylands XI, XIII. Rylands XII has seven copies including the original, Brussels 2 had five or more. Not all the figures given by Reich in *Mizraim*, 3, 44-46, are reliable.

² Reich listed 21 in *Mizraim*, 3, 44-46, without, however, giving their places of origin, which I have therefore stated above. To them add B.M. 10026 (protocol in Revillout, *Rev. égyptol.* 1, pl. 2; annotated without photograph or transcription by Glanville in *Studies presented to S. A. Cook*, Cambridge, 1950, pp. 59-60); B.M. 10027 (protocol in Revillout, op. cit. 1, pl. 1, otherwise unpublished), both from Thebes, dated 265 B.C. and 311 B.C. respectively; and the present document.

³ Literally 'writing concerning silver'. More accurately translated 'document of specie payment' by American scholars, since it is clear that in some cases no money passed, and the transaction recorded was a transfer, not a sale proper. Thus the words 'transfer' and 'transferred' would be strictly more accurate than 'sale' and 'sold' in what follows.

sons by Herieu say to Tasy daughter of Pako: 'We cede to you your house . . . and court which you bought from Herieu son of Onnoufri, his mother being Tasy, the father of Pnemhe and Herianoub aforesaid and the husband of Soptnoufri aforesaid, he having made a deed of sale and a deed of cession concerning them in favour of Pako son of Djeho, your deceased brother.'¹ It is clear in this case that Tasy's claim to own the property rests, not on a contemporary sale,² but on a past sale by Herieu to her brother Pako who had since died, and that despite the words 'which you bought from Herieu' the property had legally belonged until his death to Pako, since the deeds were made out to him, and can only have come to Tasy by inheritance from her brother.

The deeds of the former transaction thus referred to are fortunately preserved in B.M. 10616, published by Glanville twenty-five years ago.³ The remarkable point is that Herieu sold the house to Pako in October 244 B.C., over thirty years before the date of B.M. 10750. The combined evidence of the documents about the property and its neighbours may be set out in a plan:

B.C.	B.C.	B.C.
	244 The houses of Hema 213 The house of Hema	
244 The house of the barber (?) Djeho son of Onnoufri.	244 Herieu son of Onnoufri sold to Pako son of Djeho.	244 The houses of the Greeks.
213 The house of Djeho son of Onnoufri.	213 Pnemhe and Herianoub, sons of Herieu, and their mother Soptnou- fri ceded to Tasy daughter of Djeho and sister of Pako deceased.	213 The houses of the Greeks.
	244 The houses of the Greek Antipatros son of Prem- nehto. ⁴ 213 The house of Antipatros, in the hands of 'Onch- henut son of Petehor.	

Under what circumstances was B.M. 10750 drawn up? Some specific event must surely have occurred in 213 B.C. for a new deed to have been necessary after an interval of thirty years. That this event was the recent death of Pako is virtually proved by the

¹ A 3, 5-6. The sense of the word *nh̄te*, from a verbal root meaning 'to trust', appended to *P'-k' m(i')-hr̄w* is uncertain, and is discussed in note 7, p. 95.

² See p. 87, n. 3.

³ In *Griffith Studies*, 152-60. B.M. 10616 was purchased by Sir Herbert Thompson and given to the Museum in 1931 (*BMQ* 6, 6). It is not stated how he acquired it. Two documents referring to the same house must surely have been found together, and their separation must have been the work of finders or dealers. If so, some doubt is cast on the significance of the fact that B.M. 10750, B.M. 10774, and the Greek tax receipt were rolled up together.

⁴ Lit. 'the horseman'. Thompson brilliantly suggested that it translated the Greek Hippias, *op. cit.*, p. 159, n. 11.

fact that in B.M. 10750 the epithet *m(r)-hrw* 'deceased' is appended to his name alone, and not to those of others who must surely have been dead at this time, e.g. Herieu and his parents.¹ Pako's death left Tasy, his heiress, and her descendants in a weak position legally, more especially if he died intestate; for her only documentary proof against Herieu's heirs would have been B.M. 10616, which bore neither her own name nor any formal assent by Herieu's relict and heirs.²

In these circumstances Tasy herself or the executor of Pako's estate may well have approached Soptnoufri and her sons and persuaded them to execute a new deed that would more adequately protect Tasy and her posterity. On the other hand, it may have been that they took advantage of Tasy's position to lay claims against her in a court of law, and on being worsted were obliged by the court to draw up the new deed. Perhaps in favour of the latter possibility is the inclusion in B.M. 10750 of an interesting clause, not present in B.M. 10616, the provisions of which are to restrain the first parties from preventing the second party undertaking building works or alterations on the property, on pain of a fine of 30 pieces of silver; for it might be suggested that the inference from the inclusion of the clause must be that some such interference had in fact been attempted by Soptnoufri and her sons, and had led to the posited lawsuit. However, the clause occurs also in B.M. 10774 B, dated 27 years later, so that it may be that some scribes at Philadelphia customarily included this clause among the formulae of the *sh wy*, and that its presence in B.M. 10750 has no peculiar significance.³ The fine, too, seems low in comparison with the 500 pieces of silver stipulated in B.M. 10774 B if the clause was a main point in the document. It is also to be noted that when a *sh wy* was drawn up to bind a defeated party to observe the court's verdict, this fact is sometimes, though probably by no means always, stated.⁴ The omission of such a statement, together with the fact that Soptnoufri and her sons would have had a comparatively weak case against Tasy immediately after Pako's death, when the circumstances of his intestacy were well known, inclines me to believe rather that B.M. 10750 was drawn up by agreement of the parties as a part of the legal formalities necessary for the settlement of Pako's estate than that an action was brought; but no certainty is possible.

Agreement between the parties might have been easier if they were related, and this may have been the case. The plan on p. 88 shows that the fathers of Herieu and of his western neighbour Djeho were both named Onnoufri. Onnoufri was a common name; but when one recalls how often family property was divided between children in

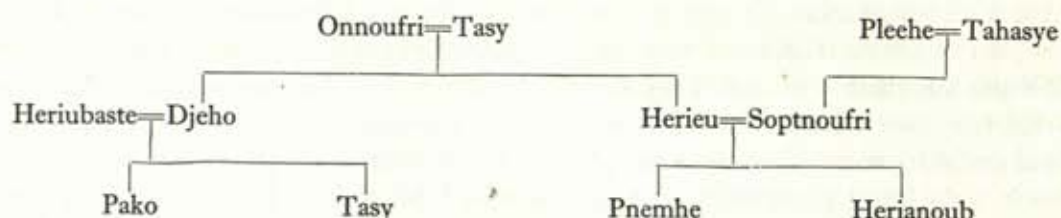
¹ See notes *r* and *dd* below.

² For agreements appended at the feet of documents by parties with a future interest in the property, see, for example, Thompson, *Siut Archive*, B vii, 3-5; B ix, 6-11; A 11: Vo. vii, 14-22.

³ I have not pursued the history of this clause thoroughly here, since Mr. A. F. Shore and I hope soon to be able to edit an unpublished document from Siut, B.M. 10589, where this formula forms the main content of the contract, which is specifically named *sh n tm sh* 'a deed of not hindering'. The fine there stipulated is 300 pieces of silver. Cf. Revillout, *Chrestomathie dém.* 213 and Glanville, *Cat. Dem. Pap. Brit. Mus.* p. 22, 10524/5-6.

⁴ e.g. Berlin 3113/3, Elephantine 12/2; also B.M. 10446 (Thebes), unpublished: *ḏd-y qnbe-t 'rme-t e'r-hr n wpyf-w n w'b-w 'mn e-tbe te-t ry't te n wpyf-w m'-t a-hr-y n'm-w* 'I have uttered a suit against you before the judges and the priests of Amūn concerning your room the judges have vindicated you against me'. On *ḏd-qnbe-t* see Griffith in *Proc. SBA* 31, 56 and Rylands, 3, 204, n. 29; also Sethe, *Bürgs.* 281, 752.

Ptolemaic times, one suspects that Herieu and Djeho may have been brothers. But there were also strong motives for re-unifying family property, and counteracting the evil effects of subdivision. One is tempted therefore to assume further that Djeho son of Onnoufri and Djeho father of Pako were the same man, and that Herieu sold out to his nephew Pako in order to re-unite his father's property.¹ If these assumptions are correct, the genealogy of the family was as follows:



There is one detail that tends to confirm this hypothetical reconstruction. If Herieu and Djeho were brothers, Tasy was called after her grandmother, as was very common; if they were not, this not very common name occurs in two unrelated families in the same document.

It remains to discuss the relation of B.M. 10750 to the documents found rolled up with it. Since B.M. 10774 concerns a half of a house only, contains no names that recur in B.M. 10750, and is dated twenty-seven years later, it does not seem that there can be a direct connexion. In the case of the Greek tax receipt, which relates to a whole house in Philadelphia and is dated in 209 B.C., it is difficult to be so confident. But if Mr. Skeat's reading $\pi\alpha\rho\alpha\ \Delta\rho$... in line 3 is correct, it is certain that the tax cannot have been paid by Tasy, though it might, if she married between 213 and 209 B.C., have been paid by her husband. At all events the interval of four years rules out the possibility that the tax can have had any connexion with the drawing up of B.M. 10750. This, together with the consideration mentioned on p. 3, n. 3, makes me think that these papyri, though doubtless found at Philadelphia by the same persons at about the same time, did not form part of a single archive.

Transliteration

The transliteration follows the text of copy A, or of B in the few places where A is damaged. The numerals in the body of the text refer to the lines of A, and the superlinear numerals to the footnotes. In these I have given the variants in full, since, although many of them are very trivial, together they serve to exhibit the measure of individuality that may be expected from scribes working at the same date and place. I have not found it possible to indicate the lacunae in the broken copies without either overburdening the footnotes, or resorting to parallel transliteration *in extenso*. I have followed the method of demotic transliteration used by Griffith, Thompson, and Glanville, in order to maintain the consistency hitherto observed by English demotists, and to facilitate comparison with B.M. 10616.

¹ The objection that Djeho, neighbour of Herieu, must have been alive in 213 B.C., whereas Pako's father was presumably dead by 244 B.C., is not valid; properties were often named after dead owners to avoid confusion, see especially Glanville, *Cat. Dem. Pap. Brit. Mus.* I, lii.

(1) *h-sp¹ 9-t 'bt-3 pr-t Pr-'o¹²² Ptlwmys¹²³ Ptlwmys¹²³ 'rme Brnyg³ n⁴ ntr-w mnh-w w'b ilgsntrs¹²⁵ n ntr-w nt nhm n ntr-w sn-w n⁶ ntr-w mnh-w n ntr-w mr-yt-w intrnqs⁷* (2) *Nygnr s-hm-t Ptlwmys-t⁸ s'-t Ptlwmys⁹ imp'tyn¹⁰ fy tn¹²¹ nb m-bh rsyn t mr-sn*

dd wy¹² P-nmh Hr¹²³ mw-t-f¹⁴ Spd-nfr-t hn' wh-mw¹⁵ Hr-'np Hr¹²³ (3) mw-t(-f)¹⁷ Spd-nfr-t¹⁸ hn' s-hm-t¹⁹ Spd-nfr²⁰ s'-t P-mr-'h²¹ mw-t-s T²-hs rm²² s 3 s hwt 2 s-hm-t I-t n s-hm-t T²-sy²³ s'-t Dd-hr mw-t-s Hr-wb's-t

te-n wwy²⁴ hr-t²⁵ n²⁶ pe-t 'y nt qt²⁷ e-f hbs sy e-f mh sb²⁸ hn' pe-f 'nh²⁹ (4) nt m-s-f³⁰ nt 'r mh-ntr I 4 n³¹ p rs p³² mh³³ hr-h mh-ntr I 4 n³⁴ p yb n p 'mn hn' pe-w 'w hy nt n t 'wt³⁵ rs tme Sbk p³⁶ 'y t³⁷ mr-sn nt e-w dd n-f t nh-t³⁸ nt hr p 't mh t hny M³-wr³⁹ p⁴⁰ tš rsyn⁴¹ nt e ne-f⁴² hyn-w

rs n p 'y (5) intptrs nt hr 'nh-hnw-t⁴³ P⁴-te-hr⁴⁴

mh p 'y Hm⁴⁵

yb n 'y-w n Wynn-w⁴⁶

'mn p 'y Dd-hr Wn-nfr⁴⁷

a mh n hy-w n hyn-w pe-t⁴⁸ 'y n pe-t⁴⁹ 'nh nt hry 'n-t⁵⁰ e-tbe ht e-'r Hr¹²³ Wn-nfr⁵² mw-t-f⁵³ T²-sy⁵⁴ (6) n⁵⁵ p yt n P-nmh hn' Hr-'np nt hry n p 'hy⁵⁶ s-hm-t Spd-nfr-t nt hry e-f 'r⁵⁷ sh e-tbe ht sh wwy a-r-w rn P⁵-k⁵⁸ Dd-hr⁵⁹ n⁶⁰ pe-t sn m(')-hrw⁶¹ nhte

mte-t n⁶² p⁶³ 'y nt hry hn' pe-f 'nh nt hry te-n wwy hr-t n 'm-w mn mte-n hp⁶⁴ wpy-w⁶⁵ (7) mt nb n⁶⁶ p t⁶⁷ e-'r n-t rn-w ty⁶⁸ p hw a-hry bn-e⁶⁹ rh rm nb n⁷⁰ p t 'nn h-n mte 'r-shy⁷¹ n 'm-w bl-t n⁷² p šr

1. In E there are damaged remains before the date, that must have given the titles and name of the witness. I cannot read them.
2. B n Pr-'o¹²².
3. B Ptlwmys sy Ptlwmys.
4. B om. n.
5. E *ilyg¹²⁵*, or more probably *ilygs¹²⁵*.
6. E 'rme n.
7. E *intrnygtes*.
8. E (Pt)lwmys (-t).
9. E Ptlwmys.
10. E *imprn'tyn*, with large n. Short stroke under p in AB perhaps also n, perhaps a space-filler.
11. B *tn¹²¹*, E *tn*.
12. BD *w'y*.
13. B Hr D sy Hr.
- BD om. 'ws.
14. B *mw-t(-f)*.
15. A damaged; probably not room for the group read *wh-mw* in B. CDE missing.
16. B sy Hr, BE om. ¹²³.
17. E *mw-t-f*.
18. B *Spd-nfr*.
19. See Note e.
20. BE *Spd-nfr-t*.
21. ADE *P-mr-'h-t*.
22. BE a rm.
23. On the reading see Note g.
24. E *wy*, C *wy* or *wwy*.
25. CD *a-hr-t*.
26. D om. n.
27. B *nt e-f qt*.
28. This phrase rubbed and partially obscured in both A and B.
29. E n ...? n(e-f wr)h; see notes 108a, 128.
30. On reading of B see note j.
31. E om. n.
32. BE n p.
33. The appearance of *mh* and 'mn a few words on in B suggests that the scribe may have originally written each in the place of the other and later corrected without troubling to erase.
34. BE perhaps om. n.
35. B 'we DE damaged.
36. B p doubtful.
37. B om. t, which in A has coalesced with det. of 'y.
38. See Note m.
39. B *M-wr*.
40. B n p.
41. BC *rsyn*, E *rsn*.
42. AB damaged, e ne-f clear in E.
43. See Note n.
44. B sy P⁴-te-hr.
45. See Note o.
46. B originally wrote *yb p 'y Dd-hr Wn-nfr* in error, and has corrected clumsily without troubling to erase.
47. Contrast B's writing with writings of Hr sy Wn-nfr, B4 (end), B11.
48. BC n pe-t.
49. B hn' pe-t, E om. n.
50. B perhaps a-n-t, but the stroke is upright and rather far from 'n; more probably it is the superfluous stroke that occasionally accompanies hry.
51. B om.
- 'ws.
52. B sy Wn-nfr.
53. B *mw-t(-f)*.
54. B T²-sy-t.
55. B om. n.
56. See Note w.
57. 'r originally omitted in A, and later inserted above the line.
58. See Note q. BC n P⁵k.
59. BE sy Dd-hr.
60. BCE om. n.
61. ABC all show preliminary stroke before hrw, but E omits it. See Note r for discussion.
62. BCD om. n.
63. CD pe-t.
64. ABE a single stroke after hp, D two strokes, i.e. pl. hp-w.
65. BE *wpyt-w*.
66. BE om. n.
67. D om. mt nb n p t.
68. The small second group of ty is peculiar to documents from the Fayyūm.
69. BDE write two short meaningless strokes after bn-e, perhaps another local scribal habit.
70. BDE om. n.
71. B e-'r-shy.
72. BC om. n.

t šr-t mte-n⁷² p sn t sn-t n⁷² p rm nb n⁷³ p t' nt e-f 'y hr-t⁷⁴ n p⁷⁵ 'y p⁷⁶ 'nh nt hry rn-n e-n ty wwy-f⁷⁷ hr-t⁷⁸ e-n⁷⁹ tm ty wwy⁸⁰ e-n⁷⁹ ty wwy-f⁸¹ (8) hr-t⁸² š' hq⁸³ nb 't-dd qnb⁸⁴ nb mt nb n⁸⁵ p t' rme-t⁸⁶ mte-n⁸⁷ sh nb 'r-w a-r-w⁸⁸ hn' sh nb 'r-w (n)-n a-r-w⁸⁹ hn' sh nb 'r-w⁹⁰ n⁹¹ pe-n yt t⁹² mw-t a-r-w hn' sh nb qnb nb⁹³ nt e-n⁹⁴ m'-k⁹⁵ n'-m-w⁹⁶ rn-w⁹⁷ mte-t⁹⁸ s hn' pe-w⁹⁹ hp mte-t¹⁰⁰ p nt e-n m'-k⁹⁵ n'-m-f¹⁰¹ rn-w p 'nh p ty 'h' r t nt e-w ty s¹⁰² m-s-t (9) ty¹⁰³ 'r-n st rn-w¹⁰⁴ e-n 'r-f¹⁰⁵ pe-t ssw mr¹⁰⁶ qf¹⁰⁷ 'r hy nb pe-t 'y¹⁰⁸ n wrh-w^{108a} nt hry bn-e-n sh-t-t¹⁰⁹ n'-m-w bn-e-n¹¹⁰ ty sh-t-t¹¹¹ e-n sh-t-t¹⁰⁹ e'-r¹¹² rm nb n¹¹³ p t' sh-t-t¹¹⁴ rn-n e-n ty n-t ht 30 sttr 150 ht¹¹⁵ 30 'n p 'bt . . . m-s p 'bt rn-f¹¹⁶ n hte 't-mn¹¹⁷ a-e-t¹¹⁸ m-s-n ty¹¹⁹ (10) qf-t¹²⁰ a¹²¹ ty 'r-t¹²² hy nb n'-m-f 'n 't-dd qnb nb mt nb n¹²³ p t' rme-t e'-r-n¹²⁴ 'r n-t¹²⁵ p¹²⁶ sh wwy¹²⁷ pe-t 'y pe-t¹²⁸ 'nh nt hry a¹²⁹ m-s-n p hp p¹³⁰ sh e-the ht hn' p¹³⁰ sh wwy¹³¹ 'r¹³² Hr¹³³ Wn-nfr n¹³⁴ pe-n yt¹³⁵ rn P'-k' Dd-hr¹³⁶ pe-t¹³⁷ sn m(i')-hrw¹³⁸ sh¹³⁹ P'-te-hr-p-šr-s't S-qt¹⁴⁰ (11) e-f mte p š' nt sh a-hry

73. B om. n. 74. B a-r-t. 75. B pe-t. 76. B hn' pe-f 'nh, DE pe-t 'nh.
 77. B wy, E wy-f, D probably wwy-f. 78. D a-hr-t. 79. D a-e-n. 80. BD wwy-f hr-t, E wy-f hr-t. 81. B wy-f. 82. D om. hr-t. 83. Read by C. F. Nims. 84. B is defective. Apparently the scribe wrote 't-, and then, possibly because he took his eyes off his copy, took it to be the first group of qnb, which is not dissimilar in form. He carried straight on with the determinative of qnb, thus omitting the first part of that word and dd entirely. 85. BDE om. n. 86. B 'rme-n-t, perhaps under the influence of e'-r-n-t. 87. A mte-n in error, BD correctly mte-t. 88. B a'-r-w 'r-w. 89. B a'-r-w. 90. BC a'-r-w. 91. BDE om. n. 92. BDE te-n. 93. B adds 'r-w after nb. 94. E mte-n for nt e-n. 95. D m'. 96. DE n'-m-f. 97. E rn, apparently without suffix pronoun. 98. E mte. 99. D pe (sic). 100. B mte. 101. B n'-m-w. 102. E ligature for ty-st. 103. B a ty. 104. B 'r-s rn-n, see Note v. 105. B e'-r-f, E . 'r(-f). 106. Unusually large form in B. 107. BE qf. See Note w. 108. A writes a vertical stroke after 'y, perhaps hn', more probably error. 108a. E ne-t wrh (sic), B probably as E, D traces clearly indicating (pe-t) 'nh. 109. B sh-t D sh-ty, E sh-ty. 110. D a-bn-e-n. 111. B sh-t, D sh-ty-t rm nb p t' rn-n. 112. D a-e'-r, see Note t. 113. BD om. n. 114. B sh-t, D sh-ty-t, E sh-ty. 115. BE a ht. 116. A damaged. Reading in text given by B alone. CD missing. E om. . . . m-s p 'bt. It is certain from the length of the lacuna and the traces that A also omitted these words, see discussion in Note y. 117. A n hte 't-mn badly rubbed, but probable. The group to be seen before m-s is much more like the end of mn than e-t. 118. B a-e-t, DE e-t. A omits, see previous note and Note cc. 119. C a ty. A writes a stroke after ty, probably a filler at the end of the line. 120. A badly rubbed, B qf-t, D 'qf, E qf. 121. A missing, CD om. a. 122. A missing, BCD 'r-t, E 'r. 123. BD om. n. 124. B e'-r-n, D e-n, A damaged but certainly e'-r-n. 125. B 'r-t. 126. B py. 127. D wwy nt hry. 128. D ne-t wrh (sic), E hn' ne-t wr(. . .). 129. BD e-t, E . -t. 130. B py. 131. E wy. 132. B a'-r. 133. B sy. 134. BD om. n. 135. B sn, carelessly corrected. 136. BE sy Dd-hr. 137. A badly rubbed: B pe-n in error; CDE pe-t. 138. ADE m(i')-hrw . B hrw; see Note dd. 139. The following scribal docket in A only. 140. See Note ff.

Translation

Year 9, Phamenoth,^a of Pharaoh Ptolemy, son of Ptolemy and Berenice the gods Euergetai: the priest of Alexander and the gods Soteris and the gods Philadelphoi and the gods Euergetai and the gods Philopatores (being) Andronikos son of Nikanor: the

woman Ptolemais daughter of Ptolemy son of Empedion (being) Kanephoros before Arsinoe Philadelphos:^b

Said the labourer Pnemhe^c son of Herieu,^d his mother (being) Soptnoufri, and the choachyte Herianoub son of Herieu, his mother (being) Soptnoufri, and the woman^e Soptnoufri daughter of Pleehe, her mother (being) Tahasye,^f (being) three persons, two male, one female, to the woman Tasy^g daughter of Djeho, her mother (being) Heriubaste:

We cede to you your house, which is built and roofed with beams and fitted with a door,^h together with its court which pertains to it,ⁱ which measures fourteen divine-cubits from north to south by fourteen divine-cubits from east to west, together with (what is in) excess of those measurements,^k and is in the southern district of the town of Souchos Philadelphia,^l which is called Tnouhe^m and is on the northern side of the canal of Moeris, in the nome of Arsinoe; the boundaries of which are: south, the house of Antipatros, which is in the possession of Onchhenutⁿ son of Petehor: north, the house of Hema:^o east, the houses of the Greeks: west, the house of Djeho son of Onnoufri: which completes the survey of the boundaries^p of your house and court aforesaid, which you bought from Herieu son of Onnoufri, his mother (being) Tasy, that is, the father of Pnemhe and Herianub aforesaid and the husband of Soptnoufri aforesaid, he having made a deed of sale and a deed of cession respecting them in favour of Pako^q son of Djeho, your deceased brother.^r

Yours is the house aforesaid and its court aforesaid; we have no rights nor judgments nor any claim on earth against you in respect of them from to-day henceforth. No man on earth (nor) we ourselves likewise shall be able to exercise authority over them except you. Any son or daughter of ours or any brother or sister or any man on earth^s who shall come against you on account of the house and court aforesaid in our name, we will cause him to cede to you; if we do not cause him to cede to you, we will (nevertheless) cause him to cede to you at any cost without uttering any title or claim on earth against you.

Yours^t is every deed which has been made concerning them, and every deed which has been made for us concerning them, and every deed which was made for our father or mother concerning them, and every deed and title in respect of which we are entitled to them. Yours they are, together with the rights (conferred by) them;^u yours is that to which we are entitled in respect of them. The oath (or) the confirmation which it may be required of you that we should make in respect of them—we will make it.^v

The day that you desire to build^w or undertake any repairs on your house or its grounds (var. court) aforesaid, we will not hinder you,^x nor will we cause you to be hindered (var. cause any man on earth to hinder you in our name); if we hinder you or any man on earth hinder you in our name, we will give you 30 pieces of silver, (that is) 150 staters, making 30 pieces of silver again in the month in question (var. in the month after the month in question),^y obligatorily and without delay. We are responsible to you^z to allow you to build and to allow you to undertake any expense on it also, without uttering any title or claim on earth against you.

For you we have made ^{aa} the ^{bb} deed of cession respecting your house and your court

(var. grounds) aforesaid. We are responsible to you^{cc} for the right (conferred by) the^{bb} deed of sale and the^{bb} deed of cession, which Herieu son of Onnoufri made in favour of Pako son of Djeho, your deceased brother.^{dd}

Peteharpshenese^{ee} son of Sakote^{ff} wrote (it), he witnesses the contract which is written above.

Notes

^a April 13–May 12, 213 B.C. Documents dated by the month only are assumed to have had validity from its first day (Thompson in *Griffith Studies*, 16).

^b These priests also occur in Hauswaldt 17, where the name of the Kanephoros is damaged. The Alexander priest alone appears in Ost. Strassburg 6 (Spiegelberg, *ZAS* 50, 29).

^c Or *P-nm*; see Erichsen, *Glossar*, 218 *nm* and 219 *nmh*.

^d Mattha, *Demotic Ostraca*, 84, interpreted the final stroke of this name as the plural sign, doubtless correctly; but the writing here with *ws* (also Berlin 3089/6) proves that some scribes misinterpreted it as a 'divine' determinative.

^e For this writing of *s-hm-t* cf. Cairo 30623/1, Berlin 3079/4, Rylands X/1, Erichsen, *Glossar*, 307. Also in AE *s-hm-t T-sy*, where B has the normal writing.

^f For the reading *T'-hs*, cf. *P'-hs* in Spiegelberg, *Demot. Pap. Zenon*, 1/4.

^g *T'-sy* not *T'-hy*, cf. Cairo 10262/2 transliterated *Tasiri* (dative) in the Greek docket thereto.

^h AC *nt qt e-f hbs*, B *nt e-f qt e-f hbs* are surely arbitrary variants of the normal *nt qt hbs* (contra Glanville, *Cat. Dem. Pap. Brit. Mus.* 1, p. 7, n. i). *e-f hbs sy e-f mh sb* (damaged in all copies but certain) is more explicit than the normal *e-f mh sy sb*.

ⁱ For *nt m-s-f* 'pertaining to', see Nims in *JEA* 24, 78. In B *nt m-s-f* is hardly possible, cf. B 10, but I cannot suggest a satisfactory alternative reading. Perhaps a defective attempt to correct an error, as elsewhere in B (notes 33, 46, 84, 135).

^k Lit. 'together with their length (of) measure', i.e. plus anything over. Griffith translated 'more or less' (*Rylands*, 3, p. 132, n. 1).

^l On the writing of 'y in town names see Sethe, *Bürigs*. 482.

^m *T nh-t* 'The Sycamore-tree', less probably pl. *n nh-w*. That this is a name for the town of Philadelphia itself, not merely for its southern quarter, is proved by the masc. resumptive pronoun in *nt e-w dd n-f*, which must refer to *tme Sbk*, not to the fem. *t'wt rs*. The only other reference to this name for Philadelphia in demotic known to me occurs in a manuscript note of Thompson's: '*T nh-t*, a locality near Philadelphia: B.M. lease 15th Epiph, l. 6–7'. Unfortunately I have been unable to trace this document without the Museum number; it can hardly bear the full reading of B.M. 10750, else Thompson would have realized the identity of *T nh-t* and Philadelphia. The introduction of an Egyptian name in the reign of Philopator for a Greek town founded by Philadelphos is rather noteworthy; it may be significant that it appears neither in the demotic Zenon papyri (Philadelphos year 28–Euergetes I year 5) nor in B.M. 10616 (Euergetes I year 4). Can this indicate that there was an increase in the proportion and importance of the native population in Philadelphia during the reign of Euergetes I (Herieu's southern neighbour in 244 B.C. was a Greek, who by 213 B.C. had leased his house to an Egyptian)? For the demotic name for Philadelphia see further Spiegelberg in *Archiv*, 10, 17.

ⁿ For the reading *hntw-t* cf. Erichsen, *Glossar*, 313. Possibly *nh-hntw-t-n*, but I know of no Egyptian name containing a first person plural pronoun.

^o *Hm'*, read by Glanville in B.M. 10616, rather doubtful (note the absence of patronym); is this group really a name?

^p Lit. 'to complete the measures of the boundaries'. Glanville's translation, cf. B.M. 10616/4.

* The writings of this name are varied and bizarre (cf. A 6, 10; B 6, 11; D 12; E 7, 13) but comparison with B.M. 10616/2 proves the reading.

* ABC *pe-t sn m(i')-hrw nhte*, E *pe-t sn hrw nhte*. The reading *nhte* is certain but the interpretation is difficult. Grammatically, it seems most likely to be attached to the epithet *m(i')-hrw*; indeed E 'the voice is trusted', if the variant be not a plain error, appears to be simply a synonym for *m(i')-hrw* 'true of voice'; and one would be tempted to read ABC *a hrw nhte* and construe the same way (taking *a* as a writing of the circumstantial *e*), were it not that the group appears in the last line of the text without *nhte*, thus imposing the reading *m(i')-hrw* (Note *dd*). If *m(i')-hrw* is correct, the parsing of *nhte* is difficult; is it to be taken as a noun or infinitive governed by an omitted prepositional *n*, 'true of voice through trust, trusting (?)', or as a participle, 'true of voice, trusted'? Might it mean 'true of voice, it is hoped', i.e. potentially justified, referring to a dead man's status during the seventy days' embalming period before he was thought to have reached the judgement hall of Osiris? This would fit the fact of Pako's recent death (p. 88), but there appears to be absolutely no parallel in Egyptian literature for such a usage. The only alternative to taking *nhte* with *m(i')-hrw* seems to be to take it with the foregoing verb, 'he (Herieu) making a deed of sale and a deed of cession concerning them in favour of Pako son of Djeho, your deceased brother, in trust' or possibly 'as trustee'. Though superficially attractive, this interpretation seems to me untenable; Pako could only be holding the property 'in trust' for Tasy, and so notable a fact could surely not have been entirely omitted from B.M. 10616, even if it could have been passed over in a single word in B.M. 10750. Moreover, this technical meaning is not, I think, attested for *nhte*, and the grammar is strained. On balance I am persuaded that *nhte* amplifies *m(i')-hrw*, and that the signification of the whole phrase must be simply 'deceased'.

* *p rm nb n p t'*: for the apparently superfluous article in this phrase cf. Hauswaldt 7a/7. The reason for its presence is probably that it is felt to carry on the force of *mte-n* 'ours'; compare the instructive variant *p rm n mhw-t* 'any member of the family', in Rylands XVII/17. D probably omitted the list of relations and wrote *p nt e-f'y*.

* *A mte-n* 'ours'; this is the only real error in A. Cf. B 11 *pe-n sn* for *pe-t sn*.

* Lit. 'their right', sing., see Nims in *JNES* 7, 243a, 258b. AB perhaps nevertheless to be read *ne-w hp-w*.

* Variant in B: 'the oath (or) the confirmation which it may be required of you to make in our name—we will make it'. This is probably not a mere careless copy, but a true variant of the formula (cf. also notes x, y), and shows that these copies were probably written by professional scribes, called in as witnesses by a colleague, who knew their own versions of the formulae by rote.

* The writings of *qt* are very varied. The form '*qt*' occurs elsewhere only in the noun 'builder' *εκωτ* (Erichsen, *Glossar*, 551); cf. A 6, B 6 '*hy* for *hy* 'husband'. For the forms in B 3, 10 cf. Volten, *Studi Rosellini*, 2, pl. 35, 2/4 and Möller, *Hierat. Pal.* 3, no. 22. A 3 distorted by a vertical crack.

* There is difficulty in some of the copies in deciding whether the scribe intended to write the 2nd pers. fem. sing. suffix *-t*, since it is easily confused with the determinative; B.M. 10589 and 10774 B (see p. 89, n. 3) tend to show that A was correct in writing it everywhere.

* E *p'bt rn-f* ACD missing; measurement shows that A had the reading of E, D probably that of B *p'bt . . . m-s p'bt rn-f*; B probably intended to write *p'bt nt e m-s*, as in B.M. 10624/16 (Thompson in *JEA* 26, 72), but added a stroke in error and failed to erase it (cf. note j). Both these stipulations for the payment of fines are well attested, and even occur for separate fines within the same document (B.M. 10597/12. 14. 16, see Thompson, *Siut Archive*, 76); but it is surely very remarkable for them to occur as variants, when one appears to set the date of payment a month later than the other. Can it be that the formula of B means 'by (the beginning of) the month after the month in question', while the formula in E means 'during the month in question', and their practical effect was thus the same? Translate *ht 30* 'money, 30 (deben)', see now *Acta Or.* xxiii, 1-2, p. 133.

^z Lit. 'you are behind us'.

^{aa} B *e-r-n* (2nd present) is clearly preferable to D *e-n*, as the emphasis is on the dative *n-t* 'for you'.

^{bb} B 'this deed' (and twice similarly later in the line). D 'the aforesaid deed'.

^{cc} A *a m-s-n* certain, despite a small break. *a* for *e-t* doubtless due to the falling away of the 2nd sing. fem. suffix in pronunciation (cf. *mte* for *mte-t* in B 8, E 9); the complete omission of *e-t* in A 9 (notes 117-18) is perhaps due to the same cause.

^{dd} ADE write *m(s')-hrw* in very abbreviated form, but the reading can hardly be doubted; B *hrw* . . . , the puzzling unread group can hardly be an abbreviation for *nhite*.

^{ee} Scribal docket only in A. BCDE all omit the closing formula *sh ny* normal in witness-copies. B also omits the witness's title and name and the formula *e-f mtre* at the beginning, which is very surprising even for so careless a copy. Illegible traces of the introductory formula to E remain, and measurement shows that D also had one. Possibly these abnormalities illustrate the obsolescence of this form of document in the time of Philopator; the formulae are present in the witness-copies of B.M. 10616.

^{ff} Certainly a name and not a title, for titles when they follow a name require the definite article in demotic. The reading *-qt* is not certain in view of the determinative; cf., however, the Eighteenth Dynasty name *S-ḫd* (Ranke, *Personennamen*, 279, no. 18).



(2)



(3)



(1)











(4)

ROCK GRAFFITI IN THE EASTERN DESERT

FALCON GRAFFITI IN THE EASTERN DESERT

By P. DE BRUYN

ABOVE the plateau on which is situated the fortress of Abraq¹ there is a ridge about 40 m. high, just below the top of which there is a small natural cliff, facing east, on which is the graffito shown in pl. XIV, 1. It is 34 cm. long, the hieroglyphs reading        

stone tombs of C-group type in the Abraḳ area—two in Wādi Gehab, twenty in Wādi Hadmip near Abū Sāfi, and thirty, utterly destroyed, at Abraḳ. These sherds are mostly plain, but some have incised on them patterns composed of triangles filled with dots or hatched with lines. The colour of the ware leads me to suggest that it may have been made by a Mesopotamian technique, although the designs seem reminiscent of C-group and Pan-grave pottery.

The interpretation of the Abraḳ rock-pictures is not easy. I attribute them all to local inhabitants except for the graffiti of Dḥuṭḥotpe (*JEA* 42, 121) and Inu (pl. XIV, 1), which are evidence of the presence of Egyptians in the area. The position of the falcons shown on pl. XIV may be significant in that both face eastward towards the sunrise. The association of a falcon with cattle (pl. XIV, 2, 3) may also have significance, as well as the fact that the parti-coloured cows have white calves as near the head of Wādi Hodein.¹ Since in the latter place is a stylized human figure pulling a white calf by a rope and in pl. XIV, 2 a similar figure tows a boat carrying a falcon, it is possible that there may be a connexion between the falcon-in-boat, the sign for the god 'Anti, and the white calves. From their primitive style I am inclined to the view that the falcons and cows of pl. XIV, 2 and 3, as well as the rhinoceros, are of predynastic date, but Dr. A. J. Arkell, to whom I am indebted for help and criticism, prefers for all of them a date between the Old and New Kingdoms, attributing them all to the C-group people or their Pan-grave successors. Nevertheless, I do not see why the ancestors of the C-group should not have been established in the Eastern desert in predynastic times.

¹ Colston and Purdy, *Bull. de la Soc. khédiviale de Géogr. d'Égypte*, [2], 1886, p. 525, first noted rock pictures in Wādi Hodein.

A PTOLEMAIC INSCRIPTION FROM THERA

By P. M. FRASER

THE fragment published here is in the Museum in Thera, and was studied by me in July 1957. I owe to Professor N. Kondoleon my warmest thanks for permission to publish it. The date and place of its discovery are unknown, but it was probably acquired by the museum thirty years ago or more.

Upper part of stele of grey marble, with moulding, top and right edge preserved, rough at back. PH. 0, 12, PW. 0, 18, Th. 0, 048, letters 0,006/008, omicron 0,003/4.



[^τΕδοξ]ε τᾷ βουλᾷ καὶ τῷ δάμῳι, βουλᾶς
 [γνώ]μα· ἐπειδὴ τινες τῶν πολιτᾶν ἀνα-
 [γγέλ]λοντι ἐπὶ τὴν βουλὰν καὶ τὰν ἐκ-
 [κλησί]αν Ἀριστοκράτην καὶ Πτολεμα-
 5 [ῖον τοὺς Ἰά]σονος Ἀλεξανδρεῖς φίλου-
 [ς εἶναι τοῦ βασι]λέως Πτολεμαίου κα-
 [ὶ μεγάλας τυχεῖν προ]αγωγᾶς παρ' αὐ-
 [τῷι^ε.....]λεν εὐνου[ς]

The restoration in lines 1-4 calls for no comment. For a Theraean decree introduced by a βουλᾶς γνώμα see *IG* XII, 3, Suppl. 1291.

At the end of lines 5-6 there is no room for the final letter of either φίλου[ς] or κα[ὶ]. In line 7 προ]αγωγᾶς is obviously required, in the sense of 'preferment' or 'advancement': see the references in Holleaux, *Études*, III, 89, n. 1, which include several instances with

τυγχάνω. In line 8 -λεν is quite clear; evidently therefore the acc. + infin. construction has been replaced by another clause, either also beginning with ἐπειδὴ or else (more probably, for reasons of space) linked by a simple copula, parallel to the previous one. The sense cannot, however, be recovered with certainty, since the main verb -λεν could be restored in more than one way, for instance as [— ἀπέσται]λεν or [— ἀπ(ν)ήγγει]λεν, and more than one subject could be imagined for either verb. A supplement such as [τῶι, καὶ ὁ δεῖνα ἀπ(ν)ήγγει]λεν εὐνου[ς] εἶναι αὐτοὺς τῶι δάμωι —] fits the lacuna very well, but there are objections to it: one would expect the main verb to be in the present tense; it is difficult to see who the subject of the verb would be; and αὐτοὺς seems misplaced. I think it wiser therefore to leave the passage unrestored. Little is thus lost.

The hand of the inscription has no close analogies among Theraean inscriptions, but on general grounds it may be assigned to the last quarter of the third century or the early second century B.C.—the reign either of Philopator or Epiphanes. Although Thera remained Ptolemaic until 146 B.C.,¹ and a good deal of the epigraphical material belongs to the reign of Philometor,² the present text, if correctly dated, is the only substantial evidence for the period between Euergetes I and Philometor.³ Only the preamble is preserved, but this suffices to show that the decree was of the normal honorific type. It is noteworthy that the honorands are not stated to have been envoys of the king; this may perhaps indicate that they were influential Alexandrians who were temporarily resident in the city.

Aristocrates and Ptolemaios do not seem to be known from elsewhere. The first visible letter of the patronymic is almost certainly sigma (tau is perhaps just possible, but not at all likely), and the identification with Ἀριστοκράτης Δωρίωνος Ἀλεξανδρεὺς, whose tombstone survives from Cos,⁴ is not possible. Equally unlikely is any connexion with Aristocrates, the συγγενῆς and ὑπομνηματογράφος of OGIS 163, which apparently belongs to the reign of Euergetes II or Ptolemy Alexander.⁵ The second visible letter of the patronymic is omicron, and not omega, and therefore the restoration [Ἰά]σωνος seems virtually certain, since other names in -ων almost invariably have genitives in -ωνος.⁶

¹ See Hiller, *RE*, s.v. Thera, col. 2299.

² Of the reign of Philometor are Robert, *Coll. Froehner*, 95; OGIS 59, 102, 110, 735. Of the third century are OGIS 44 (in honour of Patroklos, the admiral of Philadelphus); IG XII, 3, 328; *ibid.* 443; *ibid.* 462 (Arsinoe-plaque); IG XII, 3, Suppl. 1291 (Philadelphus); *ibid.* 1387 (Philadelphus); IG XII, Suppl., p. 87, no. 464 (Euergetes I).

³ IG XII, 3, 1389 is restored by Hiller as containing a dedication of the reign of Philopator, but Strack, *Archiv*, 2, 543, no. 20, emphasized the uncertainty of the restoration. Unfortunately I was not able to discover this fragment in Thera Museum in 1957.

⁴ Maiuri, *Nuov. Sill.* 550. The squeeze there reproduced (back to front) indicates, in addition, that the tombstone belongs to a later date, probably towards the end of the second century B.C.

⁵ See Mitford, *Opusc. Athen.* 2, 162.

⁶ I am grateful to Dr. M. N. Tod for reading my manuscript, and offering some helpful suggestions.

THE DATE OF THE EPISTLE OF BARNABAS— A DOCUMENT OF EARLY EGYPTIAN CHRISTIANITY

By L. W. BARNARD

THE tract known as the Epistle of Barnabas, which is usually classed with the writings of the Apostolic Fathers of the Church, is of unknown authorship. The consensus of opinion among scholars is that it is of Egyptian origin and most probably Alexandrian, to judge by the writer's fondness for the allegorical method of interpreting the scriptures which he may have inherited from Alexandrian Hellenistic Judaism, where it had reached its zenith in the voluminous writings of Philo. This place of origin would also account for the epistle's later popularity in Egypt. The purpose of this note is to consider the problem of the epistle's date; other questions which are of great interest to students of early Egyptian Christianity will be only incidentally mentioned.

General considerations

The epistle is clearly a tract for the times; a Jewish-Christian community somewhere or other was in danger of returning to Judaism and perhaps news of this imminent defection had been brought back to Alexandria by travellers. Our author reacts strongly and exhorts this community, whom he had visited in the past (I, 4), to remain faithful to their Christian faith: 'You ought then to understand. And this also I ask you, as being one of yourselves, and especially as loving you all above my own life; take heed to yourselves now, and be not made like unto some, heaping up your sins and saying that the covenant is both theirs and ours' (IV, 6).

The papyri indicate that there were constant contacts between the Egyptian metropolis and Middle and Upper Egypt for administrative and commercial reasons and most probably Christianity was first carried along these routes. Moreover, the early Biblical and Christian papyrus texts which come mainly from Middle Egypt indicate that the new faith had reached that area by the second century A.D., which coheres with the general probability of an outward expansion of the faith from Alexandria. The fact that Jewish communities were in existence in Middle Egypt from early times, e.g. at Arsinoë and at Oxyrhynchus to the south where a Jews' Lane is mentioned, suggests that this Jewish-Christian community was perhaps in existence somewhere in Middle Egypt in the second century A.D., although Christianity could conceivably have reached these parts at an earlier date.

We come now to the internal evidence of the epistle. Two passages come up for consideration:

I: XVI, 1-4

1. I will also speak with you about the temple, and show how the wretched men erred by putting their hope in the building, and not in the God who made them, as if it were the house of God.

2. For they consecrated Him by the temple almost as the heathen. But learn how the Lord speaks, in bringing it to naught: 'Who has measured the heaven with a span, or the earth with His outstretched hand? Have not I? saith the Lord. Heaven is my throne, and the earth is my footstool: what house will ye build for me, or what is the place of my rest?' You know that their hope was vain.

3. Furthermore He says again, 'Behold, they who destroyed this temple shall themselves build it.'

4. That is coming to pass (*γίνεται*). For through the war it was destroyed by the enemy (*ὑπὸ τῶν ἐχθρῶν*), now even the servants of the enemy will build it up again (*οἱ τῶν ἐχθρῶν ὑπηρέται ἀνοικοδομήσουσιν αὐτόν*).

Verses 3 and 4 have given rise to many conjectures allegedly bearing on the date of the epistle.

(1) Some commentators have seen here a reference to the rebuilding of the *spiritual* temple, in which case *οἱ τῶν ἐχθρῶν ὑπηρέται* is a reference to Roman officials who are taking part in the building of the true temple of God, the Church. We know that in the time of Domitian certain Christians held high positions in the Imperial Government and earlier there may have been converts in lesser positions. While it is true that the building metaphor is prominent in the New Testament¹—Christians forming the stones in the temple of the Church with Jesus as the corner-stone—nowhere else in this epistle does the author show any conception of the Church as the Body of Christ—indeed, for him the temple is the human heart (xvi, 10). Furthermore a transition in vv. 3 and 4 from the earthly temple destroyed by the Romans to the spiritual temple is unnecessarily abrupt, for the spiritual temple, i.e. the human heart, is first discussed in v. 6. For these reasons this interpretation cannot be regarded as satisfactory.

(2) That the rebuilding of an actual temple of stone is referred to is, then, probable and the question of its bearing on the date of the epistle must be raised. The supposition that the author has in mind the destruction of the first Jewish temple by Nebuchadnezzar and its rebuilding by the officials of the enemy, i.e. Cyrus and his successors (Ezra, vi, 3)—*γίνεται* being read as a historic present—is not impossible.² However, the more natural interpretation of vv. 3 and 4 is that the destruction of the temple and its rebuilding is a live question which it is necessary to speak about at length to this Jewish-Christian community, in which case the destruction of the national shrine by Titus in A.D. 70 is in view. If the epistle was then written subsequently to A.D. 70 the question is when? Vespasian, if anything, adopted an anti-Jewish policy; he ordered that the half-shekel payable to the support of the Jerusalem sanctuary should still be collected and applied to the use of the Capitoline Jupiter, an order which in the hands of Domitian became a pretext for harsh measures being directed against reculant Jews.³ There is simply no authority for the oft-repeated statement that rumours of Vespasian's intention to rebuild the temple were current among Jews during his reign. And the case is no better with Titus, Domitian, or Nerva. The only emperor who fits the bill is Hadrian (A.D. 117–38), who inaugurated a more lenient policy towards the Jews. We know that promises of the restoration of the temple were definitely made by this emperor⁴ (cf. especially *Sibyl. Or.* v, 48. 421; x, 163) and after the terrible conflict

¹ Eph. ii, 19–22; 1 Pet. ii, 5–9.

² This is favoured by A. L. Williams, *JTS* 1933, 343.

³ Suet. *Dom.* 2.

⁴ K. Thieme, *Kirche und Synagoge* (1944), 22–25. The reference cannot be to the building of the heathen

between Jews and Greeks in Alexandria and elsewhere in Egypt in the time of Trajan, which resulted in Jewish losses on a vast scale, this new policy must have appeared of great significance to Egyptian Judaism. We know that many Egyptian Jews, especially in the country areas, still looked towards Jerusalem, hellenized though they were in many ways—their own replica of the temple at Leontopolis had been closed in A.D. 73. Now there was hope that the national shrine would be rebuilt by the Romans and their officials. If this interpretation is correct then the epistle will date from early in Hadrian's reign, i.e. A.D. 118–20—a date which coheres with the general situation implied in the writing.

II: IV, 4–5

In the fourth chapter the writer warns his readers that the final stumbling-block (τὸ τέλειον σκάνδαλον) is at hand (IV, 3) and that they are living in the period immediately before the 'beloved', i.e. Jesus, will come to His inheritance. They are therefore not to return to Jewish practices or to imagine that the Jewish covenant is valid (IV, 6); rather in these last times they are to resist evil and to pay heed lest the Black One (ὁ μέλας), i.e. Satan, should get the upper hand (IV, 9). What is this final stumbling-block which Barnabas's Jewish-Christian readers are encountering? From XVI, 4 this would appear to be the hope of the rebuilding of the Temple at Jerusalem which, as we have seen, was current in certain Jewish circles in Egypt at the beginning of Hadrian's reign. The writer is apprehensive lest those who were in close contact with Jews should be led astray by these resurgent hopes. Perhaps he has in mind 2 Thess. II, 3–4, where anti-Christ, the man of sin, the son of perdition, is closely associated with the Temple. The apocalyptic setting of this chapter must be borne in mind when the crucial verses IV, 4–5 are critically examined:

- Dan.* VII, 24 And the prophet also says thus: Ten Kingdoms shall reign upon the earth and there shall rise up after them a little King, who shall subdue three of the Kings at once (or in one) (ὅψ' ἐν). Daniel says likewise concerning the same: And I beheld the fourth beast, wicked and powerful and fiercer than all the beasts of the sea, and that ten horns sprang from it, and from them a little excrescent horn, and that it subdued at once (ὅψ' ἐν) three of the great horns.

It is universally agreed that the fourth beast is the Roman Empire¹ (as in Rev. XIII. XVII; cf. Mk. XIII, 14; Ass. Moses IX, 8; 4 Ezra XII, 11–12; Hippolytus and the Talmudic text *Aboda Zara*) and that the ten horns are ten Roman emperors who have occupied the imperial throne. But that is as far as agreement goes. The attempt to penetrate the enigma further and to identify the three humbled kings has brought forth a whole crop of theories as to the date of the epistle.² The theories fall into three main groups:

temple to Jupiter Capitolinus by Hadrian after the quashing of the Jewish revolt in A.D. 135—no Jew would be interested in such an abomination; neither can the Jewish proposal to rebuild the Temple during the Bar-Chochba revolt be meant—mentioned by Chrysostom, *Hom. c. Judaeos*, v, 11; cf. Gennadius Dialogue (ed. Jahn, 1893, fol. 130^v). The reference is to a proposed Roman rebuilding in which Roman officials (and perhaps Palestinian Jews) will assist.

¹ The original interpretation of Daniel equated the beast with the Greek Empire, but this passed out of currency simply because history failed to confirm it. Cf. *Sibyl. Or.* III, 388–400.

² The 'historical allusions' in the Dead Sea scrolls are equally obscure and have produced divergent views as to their dates.

(1) The view that the ten emperors end with Vespasian, the little horn being Nero redivivus who, as anti-Christ, returns to humble Vespasian and his two sons, Titus and Domitian, who are associated with him in the exercise of supreme power, forming 'three in one'. Thus the epistle is to be dated between A.D. 75 and 79 when Vespasian died.

(2) The view that the little horn is Nerva who was elected emperor after the murder of Domitian in A.D. 96. The three humbled kings are again the three Flavians and the epistle is to be dated between A.D. 96 and 98.

(3) The view that the little horn is Nero and the three kings are Nerva, Trajan, and Hadrian who formed one family by adoption—the epistle then dating from the reign of Hadrian, A.D. 117–38.

All these theories, as they stand, have insuperable objections, quite apart from the interpretation of XVI, 3–4.

(1) places the epistle too early when its general background is examined. The most natural interpretation of *ὡς γέγραπται* (IV, 14) is that St. Matthew's Gospel is being quoted which, after allowing time for its arrival, would place the epistle not earlier than the turn of the second century; moreover, the writer may have known St. John's interpretation of the brazen serpent (XII, 6), to judge by the infrequency of this particular typological interpretation in the early Fathers. Also the fact that we must allow time for Christianity to have spread from the Egyptian metropolis to another part of Egypt indicates that the epistle should not be dated much before A.D. 100.

Against (2) it may be said that Nerva was never a great potentate and there is something odd in his humbling the three Flavians.

The objection to (3) is that Nerva, Trajan, and Hadrian were never regarded as great persecutors of the Church—indeed their leniency was dwelt upon in comparison with the cruelty of Nero and the malignant caprice of Domitian.¹ Moreover, the coupling of their names through adoption to make the 'three in one' is a little strained.

The failure of these attempts to read the Barnabas enigma suggests that another approach should be made, and the solution which will be proposed has not been put forward before, as far as we know. In the first place the writer's meaning is shown by the interpretative glosses which he puts into his quotations from Daniel. The most significant of these is the phrase *ὅφ' ἐν* which is usually taken to mean 'in or under one', i.e. the three kings who are subdued are somehow closely connected. However, *ὅφ' ἐν* simply means at once, i.e. at one blow; *ὅφ' ἐνα* would be required for 'in one'. Furthermore *ἐξ αὐτῶν* implies that the little horn is one of the ten horns, in contrast to Dan. VII, 24 where he comes *after* the ten, a strong indication that Nero is in mind.² We know that expectation of the emperor's reappearance was rife in the early Christian centuries. He was thought to be living beyond the Euphrates, whence he would swoop down and destroy his enemies (Suet. *Ner.* 57). Various impostors presented themselves as the returning emperor—one gathering followers on the banks of the Euphrates in the time of Titus, another appearing in the reign of Domitian. In the second century, according to Dion

¹ Cf. Melito addressing M. Aurelius in Eus. *H.E.* IV, 26; Tert. *Apol.* 5; Lactantius, *de Mort. Persec.* 3, 4; Eus. *H.E.* III, 31–33; Sulp. *Chron.* II, 31.

² Cf. also Dan. VII, 8.

Chrysostom, 'to the present time all men desire him to be alive, and the majority even trust that he is' (*Orat.* xxi). This belief chimed in with the Christian expectation of the appearance of anti-Christ in the last days, cf. Rev. xvii, 8; Asc. Isa. iv, 2 ff. Jewish speculation also concerned itself with Nero redivivus, for in two of the Sibylline Oracles, one certainly written in Egypt, he is expected to precede the advent of the Messianic reign as the final scourge. The belief lingered for centuries and is mentioned by St. Jerome, St. Augustine, and St. Martin of Tours.

If, then, the little horn is Nero returning as anti-Christ, who are the ten kings and the three of their number who are to be crushed by him? It is here that Rev. xvii is significant. We have to remember that our author was writing against an apocalyptic background much as was the seer of the Book of Revelation; he believed that he was living in the last times when the power of evil was everywhere in the ascendant and the rumours of the rebuilding of the Jerusalem sanctuary were the last manifestation of this evil. In such times strict logic does not apply and the Christian mind tends to view events in terms of black and white. This is shown in Rev. xvii, 10, where the seer jumps from Nero to Vespasian in his enumeration of the Roman emperors.¹ He does this because the reigns of Galba, Otho, and Vitellius were of no import in the struggle between the Empire and the Church.

With Vespasian, however, the Flavian house entered on a period of power which lasted for more than twenty-five years (A.D. 69-96), and the question which concerns us is whether Vespasian was a persecutor of the Church. Sir William Ramsay² believed that he was, although other Roman historians have adopted a more cautious attitude. One fact, which is often forgotten, is that no systematic records were kept of the early persecutions and often the knowledge possessed by writers, whether pagan or Christian, was accidental and fragmentary. Because of this the argument from silence is precarious in the extreme. The record of the sharp persecution of the Church in the reign of Trajan was only preserved accidentally for posterity and might easily have been lost. In the case of Vespasian (A.D. 69-79), a stocky, common-sense countryman who restored the prosperity of the Roman world, Hilary of Poitiers³ ranks this emperor between Nero and Decius as a persecutor of the faith. It is no argument to say that this is counterbalanced by the omission by Melito⁴ and Tertullian⁵ of any such ascription; Melito equally exempts Trajan and Antoninus Pius, both persecutors, and Tertullian exculpates Marcus Aurelius although many Christians were martyred in Vienne and Lyons in his reign.⁶ As no systematic records were kept, the knowledge of each writer must be judged on its merits: it is by no means improbable that Hilary may have preserved an historical tradition that the first of the Flavians persecuted the Church—a tradition that escaped Eusebius—although, of course, it is also possible that Hilary may have meant Domitian. In this connexion the opinion of the great Roman historian Hugh Last is significant:⁷

¹ H. B. Swete, *The Apocalypse of St. John*, 217. οἱ πάντες ἐπεσάν refer to Augustus, Tiberius, Caligula, Claudius, and Nero; ὁ εἰς ἔστιν refers to Vespasian; ὁ ἄλλος οὕπω ἦλθεν refers to Titus. In like manner the ancient Babylonian king-lists omitted kings thought to be unimportant.

² *The Church in the Roman Empire*, 253-6.

⁴ Eus. *H.E.* iv, 26.

⁷ *JRS* 1937, 80-92.

⁵ *Apol.* 5.

³ Hil. *Pictav. c. Arian.*, ch. 3.

⁶ Eus. *H.E.* v, 1.

while sceptical of Sir W. Ramsay's view, Last emphasized the change which came over the attitude of the Imperial Government between the time of Nero and Trajan. He believed that in the last quarter of the first century Judaism and Christianity became aware of themselves as two religions, and that as Imperial knowledge of Christianity became more precise, a more clearly defined policy came into existence—a policy which is in evidence in the Pliny–Trajan correspondence.

There is, too, the interesting passage in Severus Sulpicius,¹ probably derived from the lost *Histories* of Tacitus, which describes a council of war held after the fall of Jerusalem. Different opinions were then expressed about the Temple. Some thought that the Temple should be left standing; Titus and others expressed the view that the building should be destroyed so that the religions of the Jews and Christians might be more completely extirpated, for these religions had the same origin. It is unlikely that this speech embodies the *ipsissima verba* of Titus;² in accordance with ancient practice it is most probably the composition of Tacitus himself.³ Yet if this is the case its value is the greater, for it then embodies Tacitus' conception of the nature of the Flavian policy towards Christianity. In the speech the difference between Judaism and Christianity is recognized—both are evils to be stamped out—although it is not yet fully appreciated that Christianity is a religion independent of the Jerusalem Temple and cultus. However, the enmity which the speech embodies is a fitting prelude to Tacitus' account of the attitude of subsequent emperors towards the adherents of the new faith.

With the last of the Flavians there is more certain evidence of persecution. Domitian (A.D. 81–96), an embittered and jealous man, accentuated the absolutist tendencies of Vespasian, even wearing the dress of *Triumphator* in the senate. Martial curtly dismisses his reign as counterbalancing the good that Vespasian and Titus had done: 'Flavia gens quantum tibi tertius abstulit heres, Paene fuit tanti non habuisse duos.' Domitian was fond of oriental flattery and was accorded, and accepted, divine honours in his lifetime—an act which would have made him anathema to the Christians. Towards the end of his reign (A.D. 93–96) this emperor's policy became more ruthless and the evidence that he persecuted the Church, as well as the Jews, is unequivocal, the names of two of his victims in Rome being preserved.⁴ In Christian eyes this emperor, rightly or wrongly, came to rank with Nero as a great persecutor. In the Apocalypse the Beast from the sea is the hostile world-power represented by Nero and Domitian; Melito of Sardis, Tertullian, Juvenal, and many subsequent writers coupled them together.⁵ The impression made by the ruthless Domitian imprinted itself deeply on the Christian mind.

¹ *Chron.* II, 30: At contra alii et Titus ipse evertendum in primis templum censebant, quo plenius Judaeorum et Christianorum religio tollatur: quippe has religiones, licet contrarias sibi, isdem tamen ab auctoribus projectas: Christianos ex Judaeis extitisse: radice sublata stirpem facile perituram.

² Titus, a charming and intelligent man, died as 'the darling of the human race' (Suetonius) and was at once deified.

³ Cf. the speeches in Thucydides and in Luke–Acts which are likewise compositions of the authors.

⁴ Domitilla and Flavius Clemens. Acilius Glabrio was possibly another.

⁵ See the catena of passages quoted in J. B. Lightfoot, *St. Clement*, I, 104 ff., where full references are given. What happened in Rome in the time of Domitian would soon be known in Alexandria.

In the light of this fact the three humbled kings of Barn. iv, 4-5 cannot be other than the three Flavians—Vespasian, Titus, and Domitian. Only these three, in Christian eyes, were worth humbling, for Nerva, Trajan, and Hadrian, as we have seen, were not regarded as persecutors by second-century Christian writers. Only the three Flavians fill the bill. If this is the case, we suggest that our author enumerated the ten emperors in this way. Like the author of Revelation he put aside the name of Julius Caesar who, though he claimed the *praenomen Imperatoris*,¹ was a dictator rather than an *Imperator* in the later sense: beginning with Augustus he enumerated Tiberius, Caligula, Claudius, Nero; then as in Rev. xvii, 10 he omitted Galba, Otho, and Vitellius (whose reigns cover but nineteen months) as unworthy of ranking with the Augusti—resuming with Vespasian, Titus, Domitian, Nerva, and Trajan. It is a mistake to imagine that the author of Barnabas regards the little horn, Nero redivivus, as crushing the three emperors who immediately *precede* his own advent. He merely states that three of the great horns will be humbled—not necessarily the last three.² That the author's thought is supra-historical rather than historical is shown by iv, 12, cf. xxi, 3, where he speaks of the final judgement of the world when each will receive, according to his deeds, a reward of righteousness or iniquity. It therefore seems likely that in iv, 4-5 the writer envisages the ten Roman emperors resurrected and standing before the judgement throne to receive their due. The persecutors of the Flavian dynasty—and especially the arch-fiend Domitian—will be humbled at one blow by Nero who will then himself apparently be subdued by the returning Jesus. In this way a satisfactory account is given of the three humbled kings as seen through the eyes of Christian apocalyptic.

If the observations of this note are not without substance then the Epistle of Barnabas will have been written very early in the reign of Hadrian when expectations of a rebuilding of the national shrine at Jerusalem were rife among Egyptian Jews. The writer looks back over the past and finds a fulfilment of Daniel's prophecy in the ten emperors down to the beginning of Hadrian's reign who were worth consideration in Christian eyes, with the Flavian dynasty marked out for punishment.

¹ Suet. *Jul.* 76.

² The writer freely adapts the LXX of Daniel. In the original the little horn is Antiochus Epiphanes, who disposes of his last three rivals.

BIBLIOGRAPHY: GRAECO-ROMAN EGYPT

Greek Inscriptions (1957)

By P. M. FRASER

I. *Bibliography*

- (1) My summary of the year 1956 appeared in *JEA* 43, 105-15.
- (2) J. LECLANT's survey of 'Fouilles et travaux en Égypte 1955-57' appeared in *Orientalia*, 27, 76-101.
- (3) A bibliography of A. Calderini appeared in *Studi in onore di A. Calderini e R. Paribeni*, I, xxv-lviii.

II. *New Texts*

(4) In *Amer. Num. Soc. Museum Notes*, 7, 91-93, M. LANG publishes 'A Roman weight from Egypt'. This consists of a square plaque of bronze of a familiar type, containing two inscriptions: (a) on the upper part of the top surface, Λ δ αὐτοκράτορος Καίσαρος | Νέρωνα Τραϊανοῦ Σεβαστοῦ | Γερμανικοῦ, ἐπὶ Γαίον Μινικίου Ἰταλοῦ ἡγεμόνος, i.e. A.D. 100/1, and (b) on the lower part of the top surface a series of abbreviations representing λί(τραί) 1, ο(ὐ)γ(γίαι) 1½, γρ(άμματα) 6, i.e. 373.19 gr. She compares the similar weights from Egypt, *IGRR* I, 1374 and 1379, and suggests that all three may be Alexandrian librai marked with their Italic equivalents.

(5) In *JEA* 43, 70, D. MEREDITH publishes from a copy of Wilkinson a dedicatory inscription of a bust of Sarapis from Berenice Troglodytica, inscribed Διὶ Ἡλίῳ μεγάλῳ Σαράπιδι (cf. below, no. (40)).

(6) In *Studi in onore di A. Calderini e R. Paribeni*, II, 479-489, S. DONADONI publishes Greek and Coptic inscriptions from the Italian excavations at Antinoupolis, and a few graffiti from the adjacent desert. From Dēr el-Dik, near Antinoupolis, he records a graffito in a cave, evidently once a place of refuge, which gives the opening lines of St. John's Gospel. He also publishes (485 ff.) a number of stamped amphora-stoppers of late date, two of which bear inscriptions indicating that the wine was produced locally, no doubt on land owned by a monastery, in vineyards denominated by their geographical position: ἀπὸ λιῶ(του), βορρ(ᾶ), while in a third, αψιθ, D. sees a reference to αψιθ, i.e. ἀψιθ(ίτης), the wine medicated with absinth mentioned by Pliny, *NH* XIV, 107, and in λαδ an abbreviation for λαδ(ανίτης), wine flavoured with laudanum, hitherto unrecorded.

(7) In his article, *JDAI* 70, 129-154, 'Das Verhältnis der megarischen Becher zum alexandrinischen Kunsthandwerk', K. PARLASCA records (148, n. 121) the inscription on a Hadra vase in Cyprus Museum, Ζηνοδότου.

(8) In *Jew. Quart. Rev.* 48, 6-12, A. SCHEIBER republishes a Jewish terracotta mezuzah (an inscribed amulet attached to the doorposts of a house to protect it from evil spirits) in the Egyptian collection of the Hungarian Museum of Fine Arts. One side is inscribed εἰς θεός; the second ΑΤΕΟΟ, which S. interprets as ἄγιος, i.e. ἅγιος; the third ΟΕΧΘΟΝ, which S. explains as ὁ βροθηθῶν, while the fourth bears a representation of a palm-leaf. The provenance of the object is unknown, but I include it here in view of its possible Egyptian origin.

Cf. also below (9) and (13).

III. *Studies of previously published Inscriptions*

(9) The republication by A. BERNAND and O. MASSON, in *REG* 70, 1-46, of the Greek graffiti of Abu Simbel is very welcome. The republication is based on a fresh collation in 1956 by Bernand—the first since that of Lepsius in 1844. Unfortunately, present circumstances prevented the publication of the relevant photographs, and it is to be hoped that this will soon prove possible. In any case, the revision marks a substantial advance in the study of the graffiti, though the accuracy of Lepsius's copies is, as so often, vindicated at every turn. The graffiti total 33: 7, long familiar, commemorating the campaign of Psammetichus II, and 26 dated on palaeographical grounds to the Ptolemaic period. The texts are accompanied by detailed grammatical and linguistic notes, and I cannot enter here into a full discussion of the work. I note only a few points. In no. (2) (*Syll.*³ 1 (i)) they unite into one text three graffiti taken as separate by Lepsius, and recover in l. 2 of it further reference to Amasis, who also occurs in (1), although their copy shows no trace

of the *Ἀμασις* given in the transcription. Their note says 'Les mots *Ἀμασις* *ἡμα* ne sont lisible que sur l'estampage où les grains de sable, incrustés dans la gravure, sont apparus sur le papier'. Surely this word should be 'dotted' in these circumstances, or at least followed by a mark of interrogation? Furthermore, according to my own reckoning, from their drawing, there is room for about 14 letters even of the larger type which follow, and this space would be very well filled by *Ποτασίμτο* *ἡμα* while *Ἀμασις* *ἡμα* would be very short. Since the central group of both names, *Ποτασίμτο* and *Ἀμασις* is identical, I think a strong case can be made out for the longer name. In (3) (ibid. 1 (b)) they correct Lepsius's *Ελεσιβυς* to 'Ελεσίβιος, conjectured long ago by Kirchhoff; in (6) (ibid. 1 (h)) they read an initial line consisting of *καί* which explains the plural *ἐγρα(ψ)αν* of l. 2. (8)–(32) are graffiti of the Ptolemaic period. (8) (*SB* 4165): they read a full patronymic in l. 1, *Διογνήτου*, for Lepsius's *ΔΙ*, and a new l. 2, not read by L., consisting of some letters which they do not transcribe, and of which they say only 'l'interprétation paraît difficile'. (9)–(13), (15)–(17), (21), (23), (32) are all hitherto unpublished, but consist of single names, fragmentary or complete, and call for no comment. (14) stands revealed as *Σίρωνος* 'Ινδός, company, therefore, for the conjectural Indian at El Kanāyis, *Archiv.* 3, 320. Of the new reading they very reasonably say 'Le mot *Σίρωνος* nous paraît énigmatique'. I wonder how certain the final sigma is: L. read 'Ινδ[ο]ῦ, and *Σίρωνος* 'Ινδοῦ is straightforward enough, though the other graffiti are in the nominative. (19) (*Syll.* 3 1 (f–g)), previously attributed (in a very different form) to the archaic group, appears now as Ptolemaic, *Τίμος* ὁ *Πασίωνος* ὁ *ὑφιππάρχης*, the last word, if correctly read, *addendum lexicis*. (24) is new, and read as *Διονύσι[ος]* *Ἀθην[αῖος]*: perhaps read *Ἀθην[αίου]*, since other persons with Greek city-ethnics here have the patronymic (see (24), (26), (27), (29)). The fact that, as the authors point out on 40, *Διονύσιος* *Ἀσκληπιάδου* *Ἀθηναῖος* occurs in a graffito of the Syringes, and that an Asclepiades occurs in (13) is hardly cogent. In (29) *Βούτρος* *Μενελάου* *Κουρεῦς* they point out that *Κουρεῦς* is apparently a genuine variant of *Κουριεύς* (which occurs in (27)). On 39 ff. they summarize the information gained from the graffiti, and 43 ff. give useful indexes including (a valuable innovation) 'noms à supprimer'.

(10) In *BSA* 51, 55–62, J. BOARDMAN writes on 'Chian and Naucratis', studies the fabric of these two categories of vases (if they are two), and discusses the technique of the fragmentary votive inscriptions on the vases. He corrects (56, n. 5), and adds to, the list published by Cook and Woodhead in *BSA* 47 (cf. *JEA* 40, 125, no. (7)), and suggests, to explain the apparent fact, noted partially by the latter, that the same 'writer' inscribed numerous dedications made by the same man on different vases of the same style to a single deity, 'that the votives were ordered, made and inscribed in *batches* for the same man, whose offering would then be of several vases at once . . .'. He points out that the names of the dedicants do not enable us to assign the manufacture of the vases to Chios rather than to Naucratis.

(11) In *Studi Calderini-Paribeni*, II, 33–49, S. BOSTICCO publishes 'Due frammenti di orologi solari egiziani', both of which bear hieroglyphic inscriptions. He republishes in this context, from the article of J. Clédat, *Rec. trav.* 37, 38–39 (cf. also ibid. 38, 1–7 and 70–84), the early Hellenistic horologion with the names of the months in Greek, found at Qantara (*SB* 7019).

(12) In *JEA* 42, 122–3, W. R. CHALMERS republishes, with a photograph, the interesting Christian inscription, probably of A.D. 641, from an unknown site in Egypt, previously published by F. M. HEICHELHEIM, *JEA* 30, 76–77, referring to a *χαλκευτικὸν ἐργαστήριον* in the vicinity of a church. In l. 2 he confirms a suggestion of E. G. Turner that the correct reading is *ἐπισκ(όπω) τῷ* and in ll. 11–12, for the date, Heichelheim's *ἐν μηνὶ Φαῶφι τῆς δ' ἡμέρας*, *ἡδικοτ(ίονος)* *ι*, becomes *ἐν μηνὶ Φαῶφι τῆς δ' ἡδικοτ(ίονος)* (where (ονος) is presumably a misprint). As he says, the group after the delta is not eta but two oblique strokes, and the whole sign δ// therefore no doubt represents *τετάρτης*.

(13) G. GOYON's *Nouvelles Inscriptions rupestres du Wadi Hammamat* (Paris, 1957), though predominantly hieroglyphic, contains a republication of some Greek inscriptions. The inscriptions and graffiti are mainly the result of a small excavation carried out at the suggestion of ex-King Farouk, the aim of which was to remove a quantity of fallen earth from the sides of the wādi. G. has already referred to some of these inscriptions in an article in *Ann. Serv.* 49, 337 ff. On 46 he republishes the graffito *Ουέρονης*, Reinach, *Bull. Soc. Arch. Alex.* 13, 142. From a small temple in the north wall of the wādi (cf. 30) he publishes (no. 130) the dedicaion by 'Οροῦς *Κεφάλωνος* *χαλκεύς* (photo in *Ann. Serv.*, loc. cit. 359; cf. *OGIS* 660, for a similar dedication), previously published by Letronne, II, 443, no. CDLXXII = *CIG* 4716d⁵⁹, and also, immediately below it on the same stone, *τὸ προσκύν[η]μα Λονγίνου ἱππέος καὶ τοῦ ἵππου αὐτοῦ*, regarded as a separate

inscription in Letronne, II, 443, no. CDLXXIII = *CIG* 4716d⁵⁷, but which he takes as part of the dedication of Orses—correctly, to judge from the photograph in *Ann. Serv.*, loc. cit., which shows that all three lines were inscribed at the same time by the same hand; (for the formula cf. Letronne, II, no. CDLXXVIII (*CIG* 4716d⁵⁴, Lepsius, XII, pl. 100, no. 562), *Ἱερών<υ>μος τὸ προσκύνημα Θεωνίωνος Διδυμίων(ος)*). This inscription is said by Letronne, and in the lemma to *CIG*, and again in *IGRR* I, 1252, to be 'in valle Foakhir', i.e. in the side-valley of Wādī Hammāmāt, but, as Reinach, op. cit. 133, points out, there is a great confusion between the two wādīs in the earlier publications. Both Letronne and *CIG* 4716d⁵⁷ give ἹΠΓ.ΙΑΤ... and restore καὶ τοῦ ἱππ(ικοῦ) ἱατροῦ (ἱππιάτρον, *IGRR*), but the reading above, which I give from the photo in *Ann. Serv.*, seems quite clear. A great service would be rendered if someone were to publish a corpus—with variant readings—of the various inscriptions from the Eastern Desert. It is very difficult to determine whether a particular graffito is, or is not, published, as Reinach, op. cit. 133, has already pointed out.

(14) In *JRS* 47, 71–73 (and photo) 'Mark Antony in Alexandria', P. M. FRASER republishes *OGIS* 195, after collating the inscription in Alexandria. In ll. 2–3 he reads ἀμίμητον ἀφροδίσιος in place of the previous editors' ἀμίμητον Ἀφροδίσιος. Note that in l. 4 there is a misprint (as the photo and the accompanying discussion show): for 'ιδ' read 'ιθ'.

(15) [In *Epigraphica*, 17, 15–32, 'Replica a una "postilla"', A. TRAVERSA returns to his unfortunate publication of the Alexandrian epitaph, published by him in *Ἀντίδωρον Hugoni Henrico Paoli oblatum*, 282–322, in the face of the 'postilla' of C. GALLAVOTTI, *ibid.* 323–4 (cf. *JEA* 43, 102, no. (13)). He gives an over-enlarged photograph of a squeeze, and attempts to justify his readings, letter by letter, at great length. This defence confirms one's worst suspicions about T.'s ability to read a stone and to recognize Greek. None of his allegedly certain readings is in any way probable, and most of them are epigraphically impossible. Fortunately, the stone has been almost simultaneously republished, with a photograph, by Gallavotti himself (see below), so no further attention need be paid to Traversa's work.] In *Parola del Passato*, 12, 375–7, 'La Stele di Ammonio' C. Gallavotti republishes this inscription with considerable improvements. In particular, in l. 2 κάσπ[ε]ρίδος τέχνα οὐ τέλεσαντα βίον becomes καὶ γραφίδος (read by G. Pugliese Carratelli) τέχνας οὐ τέλεσαντα γέρας—the deceased was a scribe; in l. 4 ὦν πέρατος... μένος becomes ὦν σε πικρὸς... μίτος (read by M. Guarducci); in l. 6 ἱσταμένη [ζ]ώ(ο)ις becomes ἱσταμένη βιοτῆς; in l. 7 οἰκων ἐξ ἰδί[ων], Ἀμμῶνιε, συγγενέσιν μὲν replaces the unacceptable—Ἀμμῶνι[ακῶν], κῆμιν μὲν; in l. 15 ἀγνωστάταις τ' ἐν δαυσὶ becomes ἀγνωστάταις <σ>πονδαῖσι (read by Pugliese Carratelli); and in l. 17 τίνουι becomes γίνουι. The photograph does not enable one to control these changes with certainty, but they are no doubt essentially correct and the text now seems fairly satisfactory, although in l. 13 ἡρώεσσε should surely be ἡρώεσσι, and the allusion in ll. 9–10, πατρίδα σώζων | κάτθα[ν]ες, is still to seek.

IV. Religion

(16) I may note here, though the object is uninscribed, that F. Charbonneaux, *Hommages à Waldemar Deonna* (Collection Latomus, 28), 131–40 (and pls. 25–26), 'Sarapis et Isis et la double corne d'abondance', publishes an ivory group, c. 10 cm. high, consisting of two cornucopiae, each surmounted by a figure whom he identifies respectively as Sarapis and Isis. Sarapis has a ram's horn and is probably therefore identified in some way with Ammon. C. dates the group to the second century B.C., and compares the familiar didrachms of Philopator with the jugate Sarapis and Isis. He emphasizes the connexion between Ammon and the royal house, and traces the history of the double cornucopiae into the Imperial period.

(17) In *Jew. Quart. Rev.* 47, 221–44, E. R. GOODENOUGH writes on 'The Bosphoran inscriptions to the Most High God' (now *CIG* 683 f.). He notices a number of Jewish inscriptions from Egypt: p. 222, n. 6, he refers to *OGIS* 73–74, the two Jewish dedications from the temple of Pan at El-Kanais. (He quotes these as *CIG* 1537 and 1538, and says that the temple is at Edfu, some 40 miles away in the Nile valley; this error, which occurs also in *SB*, seems to derive from the lemma to *CIG* 4836c, etc., which says 'Apollonopoli Magna, hodie Edfu, in orientem ad templum', which is misleading if not actually incorrect.) P. 223, n. 7, the documents quoted are, of course, papyri, and not (as G.) inscriptions; for the use of θεῶ ὑψίστῳ in dedications he compares the dedicatory inscription of the synagogue at Athribis, *OGIS* 96.

(18) CHRISTIAN HABICHT's interesting and useful *Gottmenschen und griechische Städte* (Zetemata, Heft 14) I have noticed elsewhere (*Cl. Rev.* 72, 153 ff.), and have drawn attention to a few points. He is of course mainly concerned (109–25) with the cult of the Ptolemies in Greek cities outside Egypt.

(19) I may also note that in *JNES* 15, 236-40, F. JESI writes on 'L'Édit dionysiaque de Ptolémée IV Philopator', and maintains that Philopator wished to unite the various cults of Egypt, particularly those of foreign origin, under the aegis of Dionysus.

(20) In *Studi Calderini-Paribeni*, II, 401-32, 'Pantocrator', O. MONTEVECCHI examines in detail the instances of this term, including occurrences in inscriptions. She quotes, 403 ff., its appearance in the Hymn of Isidorus, *SEG* VIII, 548, of 95 B.C., as its earliest appearance. It occurs there in the feminine παντοκράτειρα (l. 2), and in the masculine form παγκράτωρ (l. 23), and at a later date in the funerary epigram from Megalepolis, *IG* V, 2, 472, l. 6, where it also refers to Isis, and again in the Mandulis hymn, *SB* 4127, cf. *HTR* 44, 227 ff. She points out that it also occurs in the form τῶν πάντων κρατοῦντι in the dedication to Zeus κατὰ πρόσταγμα Ὀσεΐριδος from the Delian Serapeum (*IG* XI, 2, 1234). She suggests that the pagan use of the term may have originated in Egypt, which would account for its frequent occurrence in LXX. On 408, n. 7, she notes Baillet, *Syringes*, 1761, where she accepts Tod's restoration, *JEA* 11, 256-8, Παν[τ]οκ[ρ]άτ[ω]ρ[ος] for the editor's παν[τ]οκ[ρ]άτ[ω]ρ. Discussing the other compounds in -κράτωρ she quotes (409) the Alexandrian dedication, Breccia, *Iscriz.* (*SB* 4275), l. 1, for κοσμοκράτωρ applied to Caracalla. She also discusses the use of the term in the magical papyri, in Christian writings, and on Christian tombstones, of which she quotes (420), *SB* 1540, 6186, and 8726, and (429, n. 39) the Christian graffito, Baillet, *Syringes*, 302.

(21) In *JRS* 47, 115-25, 'Deification and Julian', A. D. NOCK discusses the evidence for the ascription to Julian by his contemporaries of supernatural power. As an earlier precedent he refers (115) to, and translates, the dedication to Ptolemy Soter I and Berenike, θεοῖς Σωτῆροι, *Archiv.* 5, 156 f., no. 1, and (116) to the temple erected by Callicrates to Arsinoe II (not I) as Ἀφροδίτη Ζεφυρῆτις. He also refers (117, n. 1) to the invocation to Plato: Δεως ἡμῖν Πλάτων καὶ ἐνταῦθα (*OGIS* 721, ll. 10 f. (Baillet, *Syringes*, 1263, who separates this sentence from the main inscription, and regards it as possibly an independent text); this inscription is also discussed by M. N. Tod, no. (29) below, p. 134).

(22) I may also call attention here to the uninscribed naiskos-plaque with a representation of Cybele, in Cairo Museum, published by CH. PICARD, *Mon. Piot*, 49, 41-65. Cybele is seated flanked by Hermes and Hecate, and above the sloping cornices appear two groups of heraldically opposed armed warriors, whom Picard identifies as the Κουρητικὴ Τριάς. Below the representation is a frieze in a separate field, containing the figures of the twelve Olympians. P. quotes other instances of the Couretes in this guise, from Walter's article, *Jahreshefte*, 31, 53-80. He regards the plaque as Ptolemaic and suggests the Delta as a possible provenance.

(23) In *Bull. John Rylands Libr.* 39, 485-512 and 513-520, A. ROWE and B. R. REES write on 'The Great Serapeum of Alexandria'. The first part, by Rowe, deals with the archaeological evidence, the second, by Rees, with the introduction of the cult of Sarapis into Alexandria. Although I am not concerned with the archaeological evidence I may call attention to Rowe's statement that the article 'contains a revised and reasonably documented account of the archaeology of the Alexandrian Serapeum'. I wonder how many of his readers will agree with him. On 492, n. 1 he refers to the inscription published by myself, *BSA Alex.* 41, 49 ff., and *ibid.* to the lamp inscribed ὑπὲρ τοῦ οἴκου τοῦ ἀρχοντος, which he renders 'on behalf of the household of the Archon'. But it is not likely that an official (even if we knew of an official with this title in Alexandria) would be referred to anonymously in this way. I prefer to read ὑπὲρ τοῦ οἴκου τοῦ Ἀρχοντος. On 494 he refers to the inscriptions, two of early Ptolemaic and one of Roman date, found in the excavations, and published by Wace. He refers to the first of the Ptolemaic dedications, that by Asclepiod—and Euboulos to Sarapis, without reference to the improved version of it published by L. ROBERT, *BSA Alex.* 39, 130 (cf. *JEA* 38, 121, no. (30)). So much for the 'revision'. 505-7 contains a 'historical summary' of events in the history of the Serapeum, and 507-10 an inventory of various objects, Greek and Egyptian, found in and relating to that building. I have pointed out elsewhere (*Opusc. Arch.* III, 4°, p. 11, n. 6) that Rees's account of the early history of the cult of Sarapis pays no heed to the evidence provided by Callimachus fr. 191 Pf. and the diegesis, without which any discussion of the matter is inevitably incomplete and lacking the necessary foundation.

(24) In *Le Culte des souverains dans la civilisation gréco-romaine* (Bibliothèque de Théologie, III, 5) by L. CERFAUX and J. TONDRIAU we have the long-heralded work of Tondriau on ruler-cult (the share of L. Cerfaux is very small). The work is preceded by an extraordinarily full bibliography of 63 pages containing many works unfamiliar to me, and superseding T.'s numerous preceding bibliographies. The book is indeed

throughout a vast and methodical repertorium: apart from the bibliography, which has a separate index, there are appendixes containing lists of fourteen different types of heroes and deified mortals, other indexes, and finally 'compléments bibliographiques' and addenda (in which T. seems to repeat much of the evidence, apropos of the book of Habicht noted above, no. (18)), and five pages of Tables of Contents. On the other hand, the substance of the book is a synthetic narrative without critical analysis, based to a large extent on the previous articles of T. He refers in passing, in the chapter on Ptolemaic Egypt (189-227), to all relevant documents, but there is nothing in the way of interpretation, so far as I can see, which requires specific mention. At times T. has evidently paid insufficient attention to the documents he refers to so lavishly. Thus, if on p. 195 he is perhaps to be excused for not knowing *P.Hib.* 199 (even in the addenda), his notice on the Arsinoe-inscriptions from Thera and elsewhere (197) is quite inadequate: he regards them as boundary-stones, and gives no indication that the majority of scholars now regard these not as boundary-stones of land owned in some way or other by Arsinoe, but as simple dedications, and even the bibliographical note, which makes no mention of the important contributions of Segre and Mitford to the subject, is inadequate. Elsewhere, e.g. 202, n. 3, it is clear that T. has not read the article he quotes (for Peremans' article, 'Sur la titulature aulique en Égypte', has nothing to do with royal cult-titles for which T. quotes it), and again on 204 he uses the Phoenician inscription, *JEA* 26, 57-67, for purposes for which it has been shown that it cannot be used (see *JEA* 36, 82; 43, 109, no. (52)).

V. *Political and Social History, etc.*

(25) In *Aegyptus*, 36, 235-46, S. DARIS publishes 'Note per la storia dell'esercito romano in Egitto', in which he reviews some additions to our knowledge since the publication of Lesquier's *L'Armée romaine d'Égypte d'Auguste à Dioclétien* in 1917. He makes use of epigraphical evidence in passing, and on 239 mentions that the *cohors I Thrac(um)* mentioned in the Latin papyrus, *JRS* 27, 30 ff. (= *Corp. Pap. Lat.* 159), shows that Lesquier's suggested supplement in *SB* 4550, [Θηβ]αίων for the previously accepted [Θρ]άκων, is unnecessary. This is already pointed out by Bell, *JRS*, ad loc., whose note should be consulted.

(26) In *Chron. d'Ég.* 'Médecins de cour dans l'Égypte du III^e siècle avant J-C', CL. GORTEMAN includes the epigraphical evidence in a general discussion of the social and political role of doctors at the Ptolemaic court. She quotes and discusses in this connexion Breccia, *Iscriz.* 16, the dedication by Euergetes I in honour of his doctor. The name and patronymic have always been given as illegible, but from a squeeze I have been able to read Σε[νόφαν]τον Σωσικράτους, in which the name of the doctor is far from certain—Σε[νόφαν] is possible, but tau seems preferable to pi: Νέωνα (see below) is impossible. She also discusses Neon, the doctor of *PCZ* 59, 571, and suggests that he may be the Neon whose son Agathoboulos erected the statue of Sosibius at Cnidos, *OGIS* 79, but, though Cnidos was a medical centre of considerable importance, the name Neon is far too common for an identification to be plausible. On 332, n. 2, she discusses *Inscr. Délos*, 1525 (*OGIS* 104) the dedicatory inscription for Chrysermus ἐπὶ τῶν ἰατρῶν for which she accepts the now generally agreed lower date, and of which she gives a bibliography.

(27) In *Eine Ptolemäische Königsurkunde (PKroll)* (Klass.-philol. Studien, Heft 19), L. KOENEN publishes a Cologne papyrus containing excerpts from philanthropa of 163 B.C., some clauses of which are quoted in *UPZ* 111. His commentary includes illustrative material from the Rosettana (passim), from the edict of Tiberius Iulius Alexander (Temple of Hibis, II, no. 4) (esp. ll. 26-32), and from the Cypriot amnesty decree of Euergetes II (cf. below, no. (43)) and other epigraphical material.

(28) In *Rev. int. des droits de l'ant.* 3^e série, 4, 167-217, C. KUNDEREWICZ writes on the 'Évolution historique de la responsabilité des fonctionnaires dans l'Égypte ptolémaïque, romaine et byzantine' (the section on the Ptolemaic period appears in *Symbolae Raphaeli Taubenschlag dedicatae*, II (*Eos* 48, 2): *non vidi*) and discusses, apropos of penalties for fraudulent exaction of taxes, etc. (181 ff.), various clauses of the edict of Vergilius Capito (Temple of Hibis, II, no. 1) and of Ti. Iul. Alexander (ibid. no. 4), which K. seems to know only from the old publication in *OGIS*.

(29) In *JHS* 77, 132-41, M. N. TOD publishes 'Sidelights on Greek philosophers', which fills a long-felt need by providing a detailed list, modestly described by the author as 'some notes which lay no claim to completeness', of the epigraphical references to philosophers, philosophical schools, etc. His evidence includes several inscriptions from Egypt: *OGIS* 721 = Baillet, *Syringes*, 1265 (for which see Bataille, *Les Memnonia*, 172 and above, no. (20)); *SB* 6012; *CIG* 4785 = Baillet, op. cit. 1548; ibid. 4807h (Kaibel,

1017) = Baillet 562; *ibid.* 4814c = Baillet, 154; *ibid.* 4817 = Baillet 1440, including those concerning the philosophers of the Museum in the Imperial period. This is an extremely useful collection of material, though most of the Egyptian evidence is already in the recent article of A. Calderini (cf. *JEA* 43, 106, no. (33)).

(30) *Corpus Papyrorum Judaicarum*, 1, ed. by A. TCHERIKOWER in collaboration with A. FUKS, contains long prolegomena in which the history of the Jews in Egypt from the Ptolemaic to the Byzantine period is traced in detail. All the relevant inscriptions are recorded *passim*, but there is nothing which calls for comment here. In general this seems to me the best available discussion of this important topic.

VI. Prosopography

(31) In *Aegyptus*, 37, 65-70, 'A. Lappio Maximo, Prefetto d'Egitto sotto Domiziano?', A. GARZETTI proposes to identify the prefect Maximus of the Berlin Latin papyrus published by Kortenbeutel, *Berl. Abh.* 1939 (13), not with L. Laberius Maximus, attested Prefect in A.D. 83 (*ILS* 1996), but with the A. Lappius Maximus, consular colleague of C. Iavulenus Priscus attested by the Fasti Potentini (*AE* 1949, 23), whom he suggests inserting between Iulius Ursus and C. Septimius Vegetus, between, that is, A.D. 84 and Feb. 86: cf. also no. (33) below.

(32) In *Hermes*, 85, 501-504, 'Der Akarnane Aristomenes', CH. HABICHT points out that in the important decree of the Acarnanian League of 216 B.C. found at Olympia and published by him *ibid.* 86 ff. (republished as *IG*. IX, 1 (2), 583) the Ἀριστομένης Μεννεΐα Ἀλῦζιος of ll. 21-22, 64, is the same as the Acarnanian Aristomenes who played an important part at the Ptolemaic court at the end of the third century, and whose patronymic is provided by three demotic papyri in which he appears as eponymous priest of Alexander. He assesses the significance of Aristomenes' career in Egypt.

(33) In *Historia*, 6, 480-7, R. SYME discusses the career of 'C. Vibius Maximus, Prefect of Egypt', whose tenure of that post dates from Aug. 103 to Mar. 107. He suggests (483-4) that he may have been *praefectus annonae* before his prefecture. He rejects (485, n. 30) the view of J. Schwartz regarding two homogeneous prefects of the name (cf. *JEA* 40, 129, no. (21)), but he calls attention (487, n. 40) to the possibility that *CIL*, VI, 1538 may refer to the son of the prefect, whose birth is celebrated by Statius (cf. *JEA*, *ibid.*).

(34) In *Chron. d'Égypte*, 32, 147-51, 'Cleino à Délos', J. TRÉHEUX points out that there is no reason to identify the dedicant of ζωΐδια at Delos before 279 B.C. with the mistress of Philadelphus of that name.

(35) In *Bull. inst. fr.* 55, 125-40, 'Une Étude sur l'anthroponymie gréco-égyptienne du nome prosopite', J. YVOYOTTE comments on some of the names in J. Vergote's *Les Noms propres du P. Bruxelles Inv. E. 7616* (cf. *JEA* 41, 139 (44)).

(36) In *Philol.* 101, 164-6, F. ZUCKER discusses 'Σιμαρίστ(ε)ῖοι' which he restores as the name of the Alexandrian ἑταιρεία of 58 B.C., mentioned by Dio Chrys. *Ad Alex.* (30), 70, where manuscripts have Σιμαρίστοι καὶ ἑτέρα ἑταιρειῶν ὀνόματα. He points out the survival of the name, probably in the same family, from the earliest to the latest Ptolemaic period (cf. his earlier article on Simariste in *Rh. Mus.* 95, 338 ff.).

(37) The same author's article in *Symbolae Raphaeli Taubenschlag dedic.* II (*Eos*, 48, 2), 171-4, 'Personennamen in J. Scherers "Papyrus de Philadelphie"', contains, *inter alia*, a detailed note on the form Ἀρητίων.

VII. Lexicography, etc.

(38) In her article 'Dal Paganesimo al Cristianesimo: aspetti dell'evoluzione della lingua greca nei papyri dell'Egitto', *Aegyptus*, 37, 41-59, O. MONTEVECCIO discusses the pagan uses of words most familiar in the Christian and Jewish vocabulary, and in this connexion notices the prevalence of βίος, rather than ζωή, in funerary inscriptions from Egypt.

(39) I may also note here the *Rückläufiges Wörterbuch der griechischen Eigennamen* by B. HANSEN under the direction of F. DORNSEIFF (*Ber. Sachs. Akad.* 102 (4)). This is a very mechanical compilation, the material for which is drawn entirely from indexes and lexica, and not from an independent study of the sources: the main material comes from Preisigke's *Namenbuch*, Pape-Benseler's *Wörterbuch der griech. Eigennamen*, and the indexes of individual volumes of papyri. I have, at a brief glance, noticed instances of names deriving from Pape-Benseler, which have long since been relegated to limbo as a result of improved readings in the documents concerned but which occur here. Thus anyone who should need to use this work is advised to investigate carefully the source of any specific name.

VIII. *Geography*

(40) In *JEA* 43, 56-70, D. Meredith writes on 'Berenice Troglodytica' in which he elaborates previous accounts on the basis of manuscript notes of Wilkinson (1826). He refers to the relevant inscriptions, *OGIS* 70-1, *SB* 4033, 4049-50, and gives a photograph of a hydrea which may be that referred to in the Latin inscription, *ILS* 2483. He describes the temple, now largely vanished, and discusses (62) the stone with two dedicatory inscriptions, Letronne, i, 463-4, no. LIII = *CIG* 4842a (*SB* 8385), and Letronne, i, 464, no. LVI = *CIG* 4842d. The other inscriptions (69) are Breccia, *Iscriz.* 38 = *SB* 2039, and, also on a single stone, Letronne, i, 464, nos. LIV-LV. Cf. also above, no. (5).

(41) I may call attention to P. MONTET's *Géographie de l'Égypte ancienne*, i, *La Basse Égypte*, which, though concerned with the identification of sites according to their hieroglyphic names, calls when necessary on Greek literary sources.

(42) In *Chron. d'Ég.* 32, 284-312, 'Les Grecs à la découverte de l'Afrique par l'Égypte', CL. PRÉAUX gives a general account of the variable Greek knowledge of Africa south of the First Cataract. For the epigraphist there is to note her discussion (291) of the inscriptions of Abu Simbel and the extent of the campaign of Psammetichus II, in which she seems to accept the view of Sauneron and Yoyotte, for which see *JEA* 40, 134, no. (58), and her reference (310) to *SB* 302, the Ptolemaic graffito engraved in the temple of Tuthmosis III at Buhen, near Wādi Halfa.

IX. *The Ptolemaic Empire*

(43) In *Chron. d'Ég.* 32, 327-8 (cf. above, no. (26)), CL. GORTÉMAN discusses *OGIS* 42, the Coan inscription in honour of Caphisophon, and also refers to an *ineditum* from Herzog's excavations, from a reference in Heichelheim's *Auswärtige Bevölkerung*, 58.

(44) In *BCH* 80, 437-461, M.-TH. LÉNGER republishes under the title 'Décret d'amnistie de Ptolémée Évergète II et lettre aux forces armées de Chypre' the well-known inscription first published by Mitford. Her purpose is stated as 'to assemble everything which has been written on the subject . . .'. We are provided with an exhaustive bibliography both of studies on the text itself and of more general studies. Though she does not make the point very clearly, she accepts the view that the original edition of Mitford restored too short a line, since Wilhelm (subsequently followed by Mitford) showed that l. 19 must have read βασιλεὺς Πτ[ολεμαῖος ταῖς ἐν] Κύπρῳ τεταγμέναις πεζῖ[καὶς καὶ ἵππικαῖς] instead of simply πεζῖ[καὶς] at the end. She consequently restores at the end of l. 5 [τὰ ἔτι ὑπάρχοντα] for the previous [τὰ ἔτι], and in l. 9, instead of [δημιουθέντων] she suggests [ἀναληφθέντων or ἀνελημμένων εἰς τὸ βασιλικόν], and in l. 15, fin. κατ[ἀ] μηδεμίαν παρεύ[ρεσιν] instead of κατ[ἀ] παρεύ[ρεσιν]. Few will cavil with these restorations, which all are linguistically even more satisfactory than the shorter phrases. Independently of the length of line, she has a few other suggestions to make: at the end of l. 4 she prefers [καὶ γίνεσθαι] to [καὶ ἐργάζεσθαι], on the basis of a new reading by her of the parallel passage, P.Teb. 5, l. 8, where G.-H. had read [ἐργ]άζεσθαι and she suggests [γ]ίνεσθαι. In l. 12 she prefers ἀγαθεδομένων to Mitford's διαδεδομένων, and a study both of her photograph and of an earlier photograph in my possession taken by the late W. H. Buckler, suggests that she may be right, though the stone is very broken at this point. For the difficult ll. 21-27 she has no new suggestions, but discusses the earlier proposals of Wilhelm, Rehm, and others. This edition, even if it does not contribute a great deal to the text, provides a useful collection of parallel passages. Her argument, however, would have been easier to follow if she had used the recognized enumeration of lines of the text.

(45) In *Studi Calderini-Paribeni*, II, 163-87, T. B. MITFORD writes on 'Ptolemy Macron'. This is mainly a detailed analysis of the inscription originally published by him in *Archiv*, 13, 24, no. 12, and subsequently studied by Wilhelm and Segre (see *JEA* 40, 138 f., no. (84), and for a bibliography see no. (44) above, 439, n. 3), of which he establishes Citium as the provenance. He demolishes the restorations of both Wilhelm and Segre (and particularly the latter) with great cogency, and offers a solution of the complicated problem of the identification of the Ptolemy who appears in the inscription as the father of the honorand Eirene, the well-known eponymous priestess of Arsinoe III and mother of Andromachus who made the dedication. He gives strong grounds for rejecting the identification, proposed among others by H. Bengtson, of Ptolemy Macron with Ptolemy the son of Dorymenes, and after examining (177 ff.) all the possibly relevant epigraphical evidence in Cyprus, in the course of which he offers new versions of *BMI* 388, *JHS* 12, 181, no. 16, and *OGIS* 105, the latter of which he dates convincingly to 175-170, and in which he tentatively restores

[[[Πτολ]ε[μ]αῖος Πτολε[μ]αίου]] in the rasura, he comes to the conclusion that this 'Ptolemy the son of Ptolemy', the Alexandrian, is to be identified with Macron. The identification of this Ptolemy with the homonym of the Gortynian inscription, *Inscr. Cret.* IV, 208, then becomes certain. He also regards 'Ptolemy, the son of Ptolemy' as the son of the Alexandrian envoy to Delphi, Πτολεμαῖος Μάκρωνος, and thus the grandson of Macron. His conclusions on these two latter points thus correspond with, and confirm, those of Peremans and van 'T Dack in *Historia*, 3, 338-45 (cf. *JEA* 42, 113, no. (43)), an article which Mitford had apparently not seen. If the new reading of the rasura of *OGIS* 105 is correct, this conclusion can hardly be avoided, and the end of a long controversy has perhaps been reached.

(46) In *Studi Calderini-Paribeni*, 1, 101-9, A. PAGLIARO publishes 'osservazioni sul διάγραμμα di Cirene' (*SEG*, IX, 1). He is mainly concerned with the problem of the ἀθάνατα χρήματα but by way of introduction he makes some general remarks about the decree, from which it appears that like most, if not all, Italian scholars since the discovery of the document, he holds to a date in the reign of Euergetes, but it is surprising to see him maintain (101) that this date has never been validly challenged. He also adopts the view that the diagramma is a provisional document, and represents 'uno schema del compromesso concordato fra Tolemeo e i Cirenei, perchè servasse di base alle leggi che il κοινόν si dovrà dare'. Following De Sanctis, he draws attention to the stylistic and linguistic difficulties of the document, and comments in particular on ll. 6 ff., πολίτευμα δ' ἔστω οἱ μύριοι, κ.τ.λ., and the passage relating to ἀθάνατα χρήματα. For the most part his views are a restatement of those of De Sanctis and Arangio Ruiz, but with regard to the difficult καὶ ὅσοις εἰσὶ ὀφειλόμεναί μιν αἱ εἰκοσι Ἀλεξανδρείῃ σὺν τοῖς τῆς γυναικὸς μὴ ἀθανάτοις τετιμημένοις, κ.τ.λ., he rejects the view of the former that the reference is to a total of 40 minai (20 for the husband and 20 for the wife), and maintains that the phrase is elliptic and refers to those possessing movable property, including investment in loans, to the value of 20 minai, including interest on the loans, and that the 20 minai include the property of the wife. This is a very obscure passage of the text, and no single explanation seems satisfactory. I may note that I hope shortly to publish a new text of the inscription, based on a fresh collation.

(47) In *The Swedish Cyprus Expedn.* IV, 3, *The Hellenistic and Roman Periods in Cyprus*, by O. VESSBERG and A. WESTHOLM, there is a great deal of archaeological material of interest, and also a summary and historical survey by Vessberg (220-47) which contains a useful and well-digested, if brief, history of the period, in which, for the first time, full use is made in a single narrative of Mitford's numerous epigraphical studies of the later Ptolemaic period. Vessberg points out that while there exists a good deal of archaeological material for the earlier Ptolemaic period, there is very little epigraphical evidence, and that for the later period there is very little archaeological material but inscriptions abound. He summarizes the archaeological evidence on 227 ff.: 237-47 concern Roman Cyprus.

Cf. also no. (18).

X. The Egyptian Gods

(48) In *Hommages à Waldemar Deonna* (Collection Latomus, 28), 238-44, 'Isis y el Collegium Illychiniariorum del Pratum Novum (conv. Cordubensis)' A. GARCÍA Y BELLIDO publishes a statue with a Latin dedicatory inscription, *T(itus) Flavius Victor Colleg(ii) Illychiniariorum Prati Nov d.d.*' The figure is in a reclining position with a crocodile in attendance and in Isiac dress, and, in spite of the fact that Isis is not named in the inscription, there can be no doubt of the figure's identity. B. well recalls the line of P.Oxy. 1380, ἐγὼ ποταμῶν καὶ ἀνέμων καὶ θαλάσσης εἰμὶ κυρία. He rightly connects the *Illychiniariorum collegium* with the *λυχνάπτει* of Isiac worship, and in this context quotes the famous *λυχνάπτει*-inscription (cf. *JEA* 42, 108, no. (19)) and the *λυχνάπτει* of Isis in *IG*, II², 4471. He also discusses the British Museum lamp from Puteoli with the inscriptions *Εὐπλοία* and *λάβε με τὸν Ἡλιοσάραπιν* (cf. *JEA* 42, 112, no. (36)).

(49) In *Cahiers d'hist. mond.* 3, 967-84, M. A. KOROSTOVZEV writes 'A propos des objets égyptiens découverts en U.R.S.S.', and gives a valuable summary of largely inaccessible material. The article is of considerable general interest since K. emphasizes in particular the significance of the discoveries for the history of trade and of general international contacts. He discusses trade relations between Naucratis and Olbia, Panticapaion, etc., and also between Ptolemaic Egypt and the Bosphoran kingdom. He quotes in this connexion the dedication to Sarapis, Isis, and *theoi sunnaoi*, *IOSPE* 1², 5, and also another which he gives as *[Σαρά]πιδι . . . καὶ Εἰσι . . . [κα]ὶ Ποσειδῶνι* which I take to be one fragment—the right half—of *IOSPE* 1², 184 (cf. Latyshev's introductory remarks to this inscription).

(50) In *Orientalia*, 27, J. LECLANT devotes a considerable section (91-100) of his bulletin, 'Fouilles et travaux' (see above, no. (2)), to 'Découvertes d'objets égyptiens ou égyptisants hors d'Égypte'. This is a valuable repertorium. It contains a good deal of material of the Graeco-Roman period, and will be indispensable for future study of the subject. He refers (100, n. 1) to the altar from Aquileia of A.D. 172, containing the dedication Ἀρνούφης ἱερογραμματεὺς τῆς Αἰγύπτου καὶ Τερέντ(ιος) Πρεῖσκος θεῷ Ἐπιφανεῖ (*AE* 1934, 245), and gives a bibliography of it. All the material relating to the cults of the Egyptian Gods is of Imperial date.

(51) I may also note J. LECLANT's brief but useful account of the Egyptian objects discovered in continental Africa, *Bull. Soc. franç. d'Égypt.* 21, 29-39, 'Égypte-Afrique'.

(52) In *Hommages à Waldemar Deonna* (Collection Latomus, 28) R. THOUVENOT publishes a bronze bust of a deity whom he identifies as Sarapis, from Volubilis. It is worth noting as the first trace—if the identification be correct—of the worship of Sarapis in Roman Morocco.

(53) In 'La Pénétration des cultes orientaux dans les Pays-bas romains', *Rev. arch. de l'Est et du Centre-Est*, 5, 105-32, M. J. VERMASEREN refers (131) to the Isiac dedication, *CIL* XIII, 1337, found near Leyden.

BRIEF COMMUNICATIONS

Two amulets of cats

THE Langton Collection of cats includes two interesting specimens (no. 191 *a, b*) of minute proportions from the MacGregor Sale, one of carnelian, the other of lapis lazuli, which measure no more than



FIG. 1.



FIG. 2.

$\frac{1}{2}$ cm. each. In the publication of that excellent collection¹ they are described as having 'formed part of a string of six'. The author claims that they are the smallest hard-stone felines on record.

¹ *The Cat in Ancient Egypt*, by N. and B. Langton, Cambridge, 1940.

A similar object in the writer's possession, of fine blue turquoise, is of about the same size (fig. 1). The type appears to resemble (*b*) rather than (*a*) as far as can be judged by the published illustration. It is, at any rate, recumbent, and strongly stylized. The base is unmarked.

The Langton cats have been variously dated to the Middle and the New Kingdom. The earlier dating would be most consistent with the fact that turquoise, the material of the object here published for the first time, was extensively used during the Twelfth Dynasty, but much less later;¹ carnelian and lapis-lazuli beads were likewise popular during the Middle Kingdom.²



FIG. 3.

Another amulet of Bast, acquired from a private collection in London in 1957 (figs. 2, 3), is of blue faience and, apart from the broken-off ears, in good condition. The height is $1\frac{3}{16}$ in. It is of exceptionally fine quality as regards both the modelling and the glaze, which is brilliant, except at the back. The sacred animal is seated with its delicate head inclined to the right, which Langton (p. 29) calls a 'rare feature' in this class of objects. The loop for suspension is on the left, a little below the back of the head, and has two grooves. The tail, traditionally on the right, is not clearly indicated. The figure may be tentatively ascribed to the Bubastite period.

MANFRED CASSIRER

¹ Lucas, *Materials* (3rd edn.), 461.

² Ibid. 55 and 456.

The extent of *Wstwt* in the Old Kingdom

RECENT discussions of the Old Kingdom geography of Lower Nubia show considerable differences of opinion regarding the extent to which Middle Kingdom evidence can be used to determine the location, and more especially the extent, of the countries mentioned in the Sixth Dynasty itineraries.

The evidence for the extent of *Wstwt* in the M.K. consists of a passage in 'The Shipwrecked Sailor' (ll. 8-10): 'We have reached the end of *Wstwt*, we have passed Senmet (Biggah)'; and a graffito (which apparently now no longer exists) dating from year 29 of Amenemmes I at Korosko, which commemorates an expedition 'to overthrow *Wstwt*' (Säve-Söderbergh, *Ag. u. Nub.* 16, 64-65; Gardiner, *AEO* I, 74*).

Whether or not *Wstwt* had by the M.K. already attained its N.K. limit at the Second Cataract is uncertain (see, for example, Gardiner, *op. cit.* II, 271*; Yoyotte, *Bull. Inst. fr.* 52, 177, n. 2), but Säve-Söderbergh is certainly mistaken in his view (*op. cit.* 16) that 'Im MR . . . die Erweiterung des Begriffes *Wstwt* noch nicht festgestellt werden kann'. His location, on the basis of the above-cited graffito of Amenemmes I, of Korosko within the O.K. *Wstwt* is therefore open to question. It is in any case difficult to see how he reconciles such a location with his identification (*op. cit.* 28-29) of the of *Urk.* I, 127, 9 with the aqaba between Tumas and Medik, which latter lies downstream from Korosko. It would have been more consistent to have identified the 'ridges of *Irrt*' with the stretch between, for example, Ermenne and Korosko, especially since *stwt* might perhaps be more aptly applied to the broken, hilly country east of the river than to that on the west.

On the northern limit of *Wstwt* in the O.K. Säve-Söderbergh has nothing to say, presumably either because he does not consider it particularly relevant to the problem of locating *Zstwt* and *Irrt*, or perhaps because he regards *Wstwt* at this period as being confined to the district around Korosko (cf. his map, *op. cit.* 16). However, the restriction of *Wstwt* to the Korosko area would leave us without a name for the district stretching from thence northwards to the First Cataract (Gardiner, *AEO* II, 270*), since the area under Egyptian control barely extended beyond the Cataract (cf. Säve-Söderbergh, *op. cit.* 11).

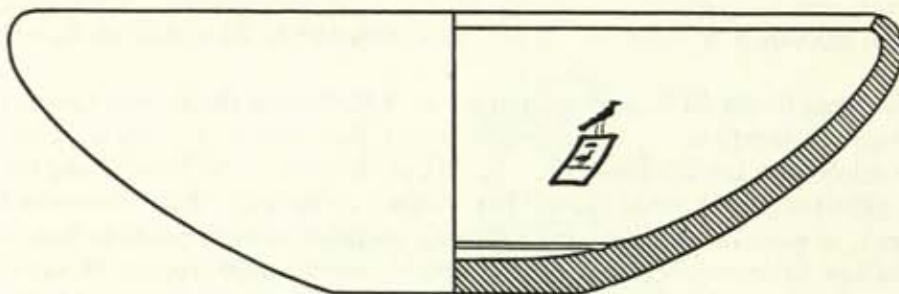
Gardiner, therefore, prefers to regard the O.K. *Wstwt* as commencing at Biggah, as in the M.K., but he does not think that it extended as far upstream as Korosko (*AEO* I, 75*; II, 270*). However, his only reason for so thinking is that he needs space between Tumas (which lay within *Irrt*) and the southern limit of *Wstwt* to accommodate *Zstwt*, which, on the basis of the order in which it is mentioned in *Urk.* I, 125, 8, he places between *Irrt* and *Wstwt*. Incidentally, Gardiner appears to have got his references confused. If he is going to locate *Zstwt* north (downstream) of *Irrt* on the basis of the order in which they occur, the passages to cite would be *Urk.* I, 126, 15 and 127, 4, not *Urk.* I, 125, 8 (so *AEO* I, 75*) which mentions *Zstwt* first, thus placing it, according to Gardiner's view, upstream of *Irrt* (cf. *AEO* II, 270* n. 2).

In the absence of any definite evidence, it seems best provisionally to regard *Wstwt* as extending upstream from the neighbourhood of Biggah, but probably no farther south than the vicinity of Seyala or Medik.

D. M. DIXON

Stone bowls of Kha'ba (Third Dynasty)

SINCE writing my communication on p. 116 of *JEA* 42, I have had the opportunity, thanks to the courtesy of Mr. William Robinow of 35 Barkston Gardens, S.W. 5, of examining carefully another fine diorite bowl with the cartouche of Kha'ba, which is in the collection made in Egypt by Mr. Robinow's father in 1895-6. In the catalogue of the collection the bowl is said to have come from Dahshūr.



Scale: bowl 1 : 2
serekh 2 : 1



The drawing of this bowl here reproduced was kindly made by Mr. H. M. Stewart of the Institute of Archaeology, London University. The *serekh* on the bowl had been filled with paint, but it has now been cleaned, and one can see clearly how it has been made by a series of drill-holes. In the frame of the *serekh*, the holes have been roughly joined by cutting, but the bodies of the two birds consist almost entirely of six and five drill-holes respectively. The general impression of the *serekh* is that it is unfinished, but it provides important evidence as to the technique of making the inscription.

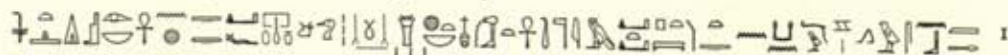
A. J. ARKELL

Motifs and phrases on funerary stelae of the later Middle Kingdom

FOLLOWING on my analysis of the *hṭp-di-nsw* in the Middle Kingdom in *JEA* 27, 77, I attempted to date many stelae of this era by placing them in one of the three periods mentioned in my analysis, viz. (1) Eleventh Dynasty; (2) early Twelfth Dynasty, i.e. from Ammenemes I to Ammenemes II; (3) later Twelfth Dynasty, i.e. from Sesostri III to the end of the dynasty.

This was done by adding up the components of a formula and comparing the totals obtained for each period. If the totals were close the inscription was omitted.

Let us take, for example, the following inscription:



¹ Brit. Mus. 286.

From it the following table can be constructed:

Component	Eleventh Dynasty	Early Twelfth Dynasty	Later Twelfth Dynasty
Osiris without determinative	10	56	94
<i>nb nh-Twcy</i>	0	0	15
<i>di-f prt-hrw</i>	0	67	94
'on which the god lives'	0	35	36
<i>kt n</i>	0	17	79
TOTAL	10	175	318

Thus comparison of the totals proves that the stela belongs to the later Twelfth Dynasty.

The result of this dating revealed that stelae of the later Twelfth Dynasty bore several new motifs in the lunette, and phrases in the *htp-di-nsw* formula. They are the following: the *wdt*-eyes,¹ the two jackals,² the *sn*-circle,³ 'that which heaven gives, earth fashions, and the Nile brings',⁴ the epithet *whm-nh*,⁵ and, finally, stelae painted entirely blue.⁶

JOHN BENNETT

A Cairo text of part of the 'Instructions of Onchsheshonqy'

THE four broken lines of wisdom text preserved on P. dem. Cairo 30682 (hereinafter C)⁷ echo passages in the 'Instructions of Onchsheshonqy' (hereinafter O),⁸ as may be seen from the following revised transliteration:

- O 7/16 *m-r hrr a šms p nt e-f šms-t-k*
 C 1 (.....) *šms-t-k*
 O 7/17 *m-r hrr a ty hpr n-k bk bk-t e-r-k rh-r-s*
 C 1 *m-r hrry* (.....)
 O 7/18 *bk e bw-r-w mhy-t-f n-š shw n ht-t-f*
 C 2 (..... *n-š*) *o shw mte-f*
 O 7/19 *rm hm e n-o te-f b-t n-š t'e-f hnt-t*
 C 2 *rm hm e n-o te-f b'(t* (.....)
 O 7/20 *rm 'o e n-hm te-f b-t n-š t'e-f hs-t*
 C 3 (.....) *n-š te-f hs-t*
 O 9/10 *m-r tnt n mt-t 'r-k 'd-t n-m-s*
 C 3 *m-r tnt n* (.....)
 O 9/14 *m-r dd sk-y n⁹ sh(t) bn-p-w 'r 'sw sk 'n n-nfr sk¹⁰*
 C 4 *m-r dd sk-y n sh(t) bn* (.....)

O 7/16–20 thus stood entire in C, which then skipped some 45–50 verses present in O.¹¹ If *m-r tnt* . . . in C 3 is correctly compared with O 9/10, which is not quite certain, as *tnt* is written differently, then C will also have omitted two of the lines O 9/11–13, for which there cannot have been

¹ E.g. Brit. Mus. 315.

² E.g. Brit. Mus. 243.

³ E.g. Brit. Mus. 227.

⁴ E.g. Brit. Mus. 312.

⁵ E.g. Brit. Mus. 304.

⁶ E.g. Brit. Mus. 223.

⁷ Spiegelberg, *Dem. Denkmäler (CCG)*, 11, 107 and pl. 50.

⁸ Glanville, *Cat. Dem. Pap. Brit. Mus.* 11, pt. i, 20–25 and pls. 7–9 (B.M. 10508).

⁹ Tentatively read as the fem. def. art. *t* by Glanville, but hardly large enough. C appears to support *n*.

¹⁰ See Glanville, *ibid.*, for translation.

¹¹ Making allowance for lines missing from the tops of cols. 7, 8, and 9.

room in C 3-4. Spiegelberg dated C 'Ptolemaic'; some of the forms¹ approximate to those of O, dated by Glanville on palaeographic grounds to the end of the first century B.C.,² while others³ probably belong to a slightly earlier stage in development, notably the writing of *m-r*, which is very reminiscent of that in the wisdom text of P. dem. Louvre 2414, dated by Volten to the second century B.C.⁴ If then, on the meagre evidence available, we accept that C is slightly earlier than O, what is the probable relation between them? One must admit that the couplet O 7/19-20 was a commonplace in Ptolemaic times, for it also occurs in L. 2414, 1/8-9 in reverse order,⁴ the latter verse being a conflation of O 14/5 and 7/19. But none of the parallels between L. 2414 and O maintain the same wording and order of sentences, as do O 7/16-20 and C 1-3. These are clearly related quite as closely as P. Insinger is to the Carlsberg fragments.⁵ In them the order and content of the verses within each chapter is much the same, but the order of the chapters occasionally varies; a similar manipulation might perhaps account for the jump in C 3 from O 7/20 to O 9/10. C must be part either of a copy of the 'Instructions of Onchsheshonqy' themselves, with certain omissions or changes in order, or of some unknown wisdom book from which the compiler of O copied slavishly. I prefer the former assumption.

H. S. SMITH

Three Coptic Etymologies

ⲙⲓⲕⲉ^{sf} Crum 161b vb. (a) intr. 'rest' (b) refl. 'rest oneself'. In the demotic 'Instructions of Onchsheshonqy' (B.M. 10508)⁶ occurs a pair of maxims: 6/18 *m-r mky ne-k ef-w bw-r-k hsy* 'Do not pamper your limbs lest you become slack': 6/19 *m-r mky-t-k r-k hl bw-r-k hsy r-k* '(-ms?) 'Do not pamper yourself when you are young lest you become slack when you are old'. The transitive use in 6/18 clearly derives from N.K. **ⲙⲕⲓ** 'protect' with parts of the body (*Wb.* II, 160, 15). But in 6/19 both usage and meaning are close to the reflexive exx. of **ⲙⲓⲕⲉ** given by Crum. If this etymology is correct, the development will have been as follows: the original transitive usage meaning 'to protect' became obsolete during the last centuries B.C. (rare in dem., Erichsen, *Glossar*, 183), but a specialized reflexive use was retained, which gave rise to an intransitive employment in Coptic.

ⲡⲩⲩⲧ^s Crum 237a nn. as pl., meaning unknown. Only ex. in MSS. Pierpont Morgan 51, 35 (parable) **ⲁⲛⲕⲱ ⲛⲉⲛⲩⲩⲧ ⲉⲣⲧⲟⲥ ⲛⲟⲩⲱⲥ ⲁⲛⲉⲛⲭⲉⲛⲱⲕⲟⲩ** (interpret) 'shelves full of books, yet we read not'. The word **ⲛⲩⲩⲧ** *nšt* f. 'hairdresser' occurs in B.M. Stela 386 (Persian period, *Wb.* II, 337, 7) and, written *nšyt*, in B.M. 10508, 10/23. Interpreting **ⲡⲩⲩⲧ** as a noun from this root meaning in the plural 'locks of hair', we may translate 'we have left our⁷ locks bleached (by the sun?) upon our⁷ cheeks, and have not braided them', which seems to provide an apt parable.

ⲱⲟⲁ^{sb}, f. **ⲱⲱⲁⲉ**^s, pl. **ⲱⲱⲟⲩ**^s, **ⲱⲱⲁⲟⲩ**^b Crum 564a nn. 'father-, mother-in-law' (Rossi, *Etymologiae Aegyptiacae* 211). The demotic ancestor of this word occurs in B.M. 10508, 9/12: *m-r hms nš 'y rme ne-k šmw-(w)·t* 'Do not stay in a house with your parents-in-law'. If Rossi's etymology is correct, the presence of the f. *·t* is surprising; conceivably it is borrowed from *mhwt* 'family', but the short final vowel of the Sah. pl. suggests that it may be correct.

H. S. SMITH

¹ C 2: *rm*, *b'(t)*; C 3: 'P.

² Glanville, op. cit. xii.

³ C 2: *hm*; C 3: *hs't*; C 4: *sh'(t)*.

⁴ Volten in *Studi Rosellini*, II, 272.

⁵ Volten, *Das dem. Weisheitsbuch*, passim.

⁶ Glanville, *Cat. Dem. Pap. Brit. Mus.* II, pt. 1, pp. 18-19.

⁷ Lit. 'the'.

⁸ Transliterated by Glanville *a*, but cf. 9/18 *hms n 'y* (op. cit., pl. 9).

The Discoverer of the Rosetta Stone: a Correction

DR. JOZEF JANSSEN has kindly called my attention to an error into which I have fallen as to the identity of the discoverer in my note, *JEA* 43, 117. My note was based partly upon a letter to his father written by an English naval lieutenant who visited Egypt in 1803. The father of the writer, a clergyman and a Greek scholar, was much interested in the Rosetta Stone, having inspected it in London, and being anxious to learn more of its history, requested his son to make inquiries on the spot during his visit to Egypt. I sent a copy of this letter to a correspondent in Paris, and asked him to make inquiry in the French Military Records and biographical resources of the Bibliothèque Nationale to obtain further information. It now appears that my correspondent has confused two nearly homonymous persons: (1) André Joseph, Baron Boussard of the Dragoons, who took part in most of the military campaigns of the period, including that in Egypt, and (2) Pierre François Xavier Bouchard (1772-1832), an officer of the Engineers, who was the real discoverer of the Rosetta Stone. I hasten to make this correction and also to point out that the place of discovery is not, as commonly stated, Fort St-Julien, but Fort Julien. The fortress was named, not after the saint, but after a young French soldier, who whilst carrying orders to the Vice-Admiral at Abukir was murdered by the Arabs.

WARREN R. DAWSON

REVIEWS

Prehistoric Religion. A Study in Prehistoric Archaeology. By E. O. JAMES. London, 1957. Pp. 300, 14 illustrations, 3 maps, 5 charts. 30s.

This is the first work, so it appears, to embrace the whole field of prehistoric religion, and only a courageous scholar who was also well equipped was likely to undertake the task. Both qualities are found in Professor E. O. James, whose studies in comparative religion now form an impressive list.

The chapter-headings will indicate the plan and scope of the work: 'Palaeolithic Burial Ritual'; 'Neolithic Burials in the Ancient East'; 'Megalithic Burial in Europe'; 'Cremation, Inhumation and Mummification'; 'Fertility and the Food Supply'; 'The Sky-religion'; and 'Prehistoric Religion'. In the final chapter, which he uses to recapitulate and sum up, Professor James puts forward the view that 'prehistoric religion centred in and developed around the three most critical and perplexing situations with which Early Man was confronted in his every-day experience—birth, death and the means of subsistence in a precarious environment'. To speak of three centres is not, perhaps, a satisfying metaphor, but the statement seems broadly to meet the evidence, provided one includes under 'birth' the emphasis on sexual energy which is shown to be conspicuous in the prehistoric data. The cult of a Mother-goddess is shown to have been widespread. It is also maintained that 'the idea of the Sky-god as the universal spirit becomes such a basic assumption that it has every appearance of having been one of the fundamental concepts of mankind'. The idea of a primitive monotheism is, however, rightly rejected, although it is admitted concerning the Sky-god that 'looked at from one angle he could be regarded as "monotheistic" in the sense of being wholly supreme and transcendent in his own domain and in respect of his divine attributes and functions'. Like Pettazzoni, Professor James believes that only the higher religions (Zoroastrianism, Judaism, Christianity, and Islam) can lay claim to monotheism, but that 'it has not evolved from polytheism or animism in the manner formerly supposed'. He is anxious to avoid any easy synthesis; and he discerns differences as readily as similarities.

Sexual fertility, as one would expect, is an earlier religious theme than the fertility of vegetation, for the latter can come to the fore only when crops are sown; the worship of the Mother-goddess is therefore in the first place unconnected with the idea of Mother Earth. Dr. Baumgartel, in *The Cultures of Prehistoric Egypt* (revised ed., London, 1955), 36, prefers to call the Egyptian form of this deity a 'fertility-goddess' rather than a 'mother-goddess' because 'she is never represented with a child'. This seems a somewhat pedantic point to make in view of the manifest child-bearing potentialities of the figures depicted; further, the term 'fertility-goddess' could easily refer to vegetation. Dr. Baumgartel (p. 46) does, however, raise one serious problem which the present work does not apparently touch upon: how is it that the prehistoric Mother-goddess in Egypt gives way so early to the idea of creation by a self-sufficient masturbating male god¹ and also to a religious royal dogma which stresses the patrilinear principle?

A difficulty inherent in any prehistoric theme is that later evidence must often be adduced in order to interpret the archaeological data. But it is open to question whether the later evidence should assume the dominant role in such exposition. Dr. James has used it very liberally. In his preface he states that 'it has been upon the Palaeolithic and Neolithic periods that attention has been primarily concentrated, where no written records obtain'; but after discussing the difficulty of defining the term Neolithic he states further that he has not hesitated 'to include the developments in the second millennium B.C. in the background of the higher religious systems of the Fertile Crescent and Western Asia, of India and of pre-Homeric Greece, calculated to throw light on their prehistoric antecedents, though in some instances the faiths may have passed into the realms of recorded history'. It is arguable that in a book which surveys so vast an expanse, a more rigid concentration on the prehistoric data would have been desirable. Certainly it is surprising to find, in the sections dealing with Egypt, detailed discussions of mummification, the pyramid cults, the ceremony of 'the Opening of the Mouth', the cult of Osiris, and the religion of Akhenaten. Nor is the discussion usually subordinated, except in the last chapter, to the investigation of possible prehistoric antecedents.

¹ Mr. R. T. Rundle Clark has recently called my attention to the fact that in de Buck, *Coffin Texts*, II, 161a the double pronoun *pn tn* ('he-she') is used to refer to Atum: 'I am he who engendered Shu; I am he-she.'

The following details would seem to call for comment. On pp. 34–35 the sequence 'Badarian, Amratian (Naqadaean) and Gerzean (Semainian)' is misleading. If Petrie's terms are followed, Semainian is subsequent to Gerzean and not identical with it; Amratian is equivalent to Naqādah I; and Naqādah II covers the Gerzean and most of the Semainian phases. In a section on 'The Mastaba Tomb and the Pyramid' the latest authority quoted for the Gizeh pyramids is Petrie's book of 1885; and for the Step Pyramid Firth and Quibell (1936); the studies of Ricke, Lauer, Junker, Edwards, and others have since shed new light on the subject. It is said on p. 43 that 'when the pharaoh of the Old Kingdom became a god in the celestial realms, the nobility in the Pyramid Age began to seek their own eternity, and so moved away their tombs from the royal cemeteries to their own domains . . .'. The truth is the opposite of this: in the Pyramid Age the nobility, as well as all the king's followers and household, were interred around him in a closely planned complex of burials: see Edwards, *The Pyramids of Egypt*, 104–5 and Junker, *Giza*, XII, 13. It is stated on p. 112 that 'the cult of Osiris came into increasing prominence' in the Fourth Dynasty; it would be interesting to have the evidence. We are told on p. 163, apropos of the 'Great Minoan Goddess', that 'Aphrodite took over her doves who became her divine son and the dynamic embodiment of her own functions'. No evidence or authority is cited in support of this remarkable view; Nilsson, whose *Minoan-Mycenaean Religion* is cited for a previous point (the first edition of 1927 is used rather than the revised edition of 1950), does not countenance it; not even Sir Arthur Evans goes so far, although he argues in *The Palace of Minos*, II, 838, for an affinity between the two goddesses.

Concerning the origin of Osiris the divergent views are scrupulously given, but one would have expected a discussion of Scharff's treatment of possible prehistoric connexions: excavations at Merimde revealed that corn had been strewn over corpses and Scharff suggests (*Forsch. u. Fortsch.* 21–23 (1947), 38 ff. and *Die Ausbreitung des Osiriskultes in der Frühzeit und während des Alten Reiches*, 17) that there is here a Neolithic parallel to the later so-called 'Corn-Osiris', except that in the early practice there is not attested an association with Osiris himself. To say, as Professor James does on p. 185, that 'with the Osirianization of the solar cult at Heliopolis the living king was equated with Horus, the posthumous son of Osiris', can be misleading, for there is sure evidence that the king before this was regarded as Horus or as Horus-Seth (as in the queen's title of the First Dynasty), but not, of course, with any Osirian connexion. For some reason *mr* *hrtw* seems to be twice (pp. 190 and 246) translated 'true of [var. in] heart and voice'.

Discussing the *Sed*-festival on p. 194, the author states that 'it is by no means clear whether it was the death and resurrection of Osiris that was impersonated, as Gardiner maintains, and Griffiths and Frankfort deny'. The reference to Gardiner's statement is *JEA* 2, 134 (actually 124), but Gardiner there takes quite the contrary view to that ascribed to him by James: he says in fact that 'there is nothing to show that the king in the *Sed*-festival was, or thereby became, an Osiris'. In *JEA* 41, 127–8, the present writer discussed the matter further. Nevertheless, it is probably right to conclude that the festival 'was closely associated with the periodic resuscitation of the king in his divine capacity'.

It is said on p. 244 that at the beginning of the Dynastic period immortality became 'the prerogative of the pharaohs'; this is contradicted by the subsequent statement, on the same page, that there is early evidence for 'a relatively advanced belief in a future life from the fifth millennium B.C. in which all were destined to share'. The latter view seems to be the correct one. Indeed, the idea of a 'royal prerogative' in relation to the after-life has been much overworked in general. While it is true that the deceased king is identified with Osiris before noblemen or commoners are, there is no evidence for an explicit or implicit royal copyright in this doctrine. It is more natural to suppose that the doctrine spread because of its popular appeal.

In a similar manner it is stated of the *ka* on p. 244 that it was originally 'exclusively a royal attribute' and that it was only in the Middle and New Kingdoms that it became 'the possessions (*sic*) of commoners as well as of the pharaohs'. Several reputable scholars could doubtless be cited in support of this opinion, but Ranke (*Personennamen*, II, 208) shows that *kr* occurs in private names of the First and Second Dynasties; one of these, a feminine name, is *Mrt-kr(i)*, which probably means 'whom (my) *ka* loves', the elliptic pronoun referring to the name-giver. Another, *Kr(i)-nhtw*, may mean '(my) *ka* is living' (see Ranke, *op. cit.* II, 210). If the addition of the suffix-pronoun be questioned, one may refer to a private name of the Old Kingdom, *Kr-i-nb-f*, 'my *ka* is his lord' (cited by Ranke, *ibid.*). The *ka* is again probably envisaged as that of the name-giver. Ursula Schweitzer in her book on *Das Wesen des Ka* (Glückstadt, 1956), 22, does not controvert this evidence, but states (p. 81) that in the archaic period ordinary men could only have a share in the *ka* of the

ruler—a conclusion which does not agree with Ranke's testimony, for how, on such an hypothesis, are these early private names to be explained? Greven, *Der Ka*, 34, and Faulkner in his review in *JEA* 41, 141, both maintain that the king was at first the only possessor of a *ka*; perhaps there is an arguable presumption that this was so if the conception arose, as it probably did, in the predynastic era; but it cannot be denied that in the Old Kingdom, as well as in the Thinite era, commoners also joined in the privilege. The main reason for the opposite view is no doubt the fact that in the Pyramid Texts the *dramatis personae* are all gods or kings; but as Černý remarks (*Ancient Egyptian Religion*, 82), much of the doctrine was doubtless believed in relation to the fate of any mortal. That private persons in the Old Kingdom were regarded as possible possessors of a *ka* is also shown by the existence of *ka*-statues in their tombs. The earliest such statue appears to have been that of Djoser; but that of the commoner Meten is not much later: see L. Greven, *Der Ka*, 33, and Ranke's discussion in *Harv. Theol. Rev.* 28, 49, concerning 'the almost simultaneous advent of such statues for king and commoner alike'.

It is not easy to agree with Professor James's belief concerning the origins of Neith, for he suggests on p. 236¹ that 'the Mother-goddess as Neith was first conceived as a cow'; reference is made to G. R. Levy, *The Gate of Horn*, 116. Miss Levy states (on p. 117) that 'she (Neith) too was a primeval cow the flesh of whose animal embodiment was taboo to the Libyans throughout their history'; but although she cites Bates, *The Eastern Libyans*, 96, n. 9 and 177, for the Libyan taboo of cow-flesh and Brugsch, *Thesaurus*, 637, 1, 8, for a late inscription naming Neith 'as Mother (or Cow)', Miss Levy does not nearly persuade the present reviewer that the goddess had originally anything at all to do with a cow. Bates does not present this view; all he does is to cite a few classical authors for the taboo of cow-flesh. Nor is there mention of Neith as a cow in *Thesaurus*, 637, 1 and 8; but in pp. 683–4 of that work Brugsch cites two such epithets as applied to Neith in late texts: *Mhī t wrt* 'the great flood', that is, the heavenly ocean thought of as a cow, and *ih t wrt* 'the great cow'. There can be little doubt, however, that Hathōr is the original possessor of both epithets; see the examples quoted by *Wb.* II, 122 (16) and I, 117 (10 and 11). Professor James rightly mentions the early occurrences of the Hathōr cow-head; he speaks (p. 236) of its 'going back to the Gerzean period and recurring in Amratian rock-drawings in Upper Egypt'. But of these two periods, the Amratian is the earlier.

The following misprints have been noted: p. 115, Anubia (Anubis); p. 185, Herakite (Herakhte); p. 200, Dodenaean (Dodonaean); p. 249, Munyas (Minyas); p. 259, χθόνιος (χθόνιος); pp. 265 and 285, Baumgärtel (Baumgartel); p. 288, Bedarian (Badarian). On p. 37, 'where the black soil brought down the river from Abyssinia' should be 'where the river brought down the black soil . . .'; p. 114, 'the viscera was separately embalmed' (were); pp. 185–6: 'Upon both of them (the Sun and the Nile), taken together, the remarkable fertility of the oasis depended': 'oasis' is a baffling word here, since Egypt as a whole is clearly meant. On p. 185 the sentence 'In the Second Dynasty the second king, Re-neb, had been given a Horus-name' should read '... the second king had been given Re-neb as a Horus-name', the theme there being the influence of the Heliopolitan solar cult.

In spite of these blemishes the book contains much that is of value, and on some major problems the author's mature judgement must command respect.

J. GWYN GRIFFITHS

Temple d'Amon à Karnak: Les Divinités des colonnes de la grande salle hypostyle et leurs épithètes. By LOUIS-A. CHRISTOPHE. Institut français d'archéologie orientale. Bibliothèque d'étude, T. XXI. Cairo, 1955. Pp. 135 and 28 plates.

The great hypostyle hall of the temple of Amūn at Karnak is said to be the largest single chamber of any temple in the world. Baikie, in his *Egyptian Antiquities in the Nile Valley*, 381, thinks that 'the columns are far too many and far too massive for the space which they obstruct rather than adorn'. Whatever one's aesthetic impression of them (the present reviewer saw them only once, enhanced by a soft moonlight), the student of religion must recognize the importance of the texts engraved upon them. The columns number 134 and they were decorated by the three kings Sethos I, Ramesses II, and Ramesses IV.

During four years spent at Karnak under the auspices of the French Institute at Cairo, M. Louis-A.

¹ As an alternative possibility he adds, 'or, as Hornblower believes, the symbolism may be of Mesopotamian origin'.

Christophe was at first engaged in forming a corpus of the inscriptions of Ramesses IV in the Theban region. He soon realized, when copying the texts of this king from the columns of the great hypostyle hall, that a deliberate choice lay behind not only the depiction of the ritual acts performed by the sovereign but also the selection of the deities represented or named. Further, there was an evident connexion between the representations belonging to Ramesses IV and those belonging to his predecessors. It was therefore decided to treat them together, and the resulting plan was to make a comparative study of the choice of deities made by the three pharaohs involved. Such a plan does not, of course, impinge upon the task, undertaken by the Oriental Institute of the University of Chicago, of publishing the texts and reliefs *in extenso*.

M. Christophe believes it is certain that the representations on the columns are bound up with the scenes engraved on the walls of the great hypostyle hall, and some of these reliefs were used by Nelson in his important study of the 'Ritual of Amenophis I' in *JNES* 8, 201 ff.; but in Christophe's opinion the columns need to be studied separately because they show a kind of abridged version of the temple ritual which was directly accessible to the priests and to others who could enter this part of the temple. Thus the idea, it appears, was to place within the reach of the lesser clergy, and possibly of the initiates and the faithful followers, the essential core of the ceremonies performed by the king or his deputy in the temple. The author thinks that it may be worth inquiring whether such an idea corresponds to an evolution of the religious life in Egypt which might have originated in the era of Amenophis IV Akhenaten.

It follows that the immediate object of the present study is to show what deities were in favour during the respective reigns of Sethos I, Ramesses II, and Ramesses IV; and to inquire whether there is discernible in the era of the Nineteenth and Twentieth Dynasties, a coherent development of religious trends. The work is divided into two parts. In the first, which is purely descriptive, the author has indicated, column by column, what deities are represented and by which king they were chosen; afterwards the deities are listed collectively under the names of the kings, and in these lists the epithets of the gods are included. The second part consists of indexes of the various component features: deities, their epithets, the divine pairs, triads, the frequency of the deities, the representations of the kings, the persons depicted in the representations, and the buildings and geographical terms. There are also three appendixes, and frequent reference is made to the plates.

All this may sound unnecessarily complex and schematic. The material, however, is not needlessly repeated. If one looks in the index of deities, for example, hoping to find under each name a list of the epithets used, one is referred to pages or paragraphs where the epithets occur. The required information is quickly available. A certain number of explanatory notes are given, especially in the index of epithets, and there are valuable references to relevant literature. But M. Christophe hopes to elaborate his considered reflections in a further study. In the meantime his painstaking and meticulous analysis of the material has enabled all students to have convenient access to it. The index of the frequency of the deities (pp. 87-88) gives at once a broad picture of the favoured emphases. It shows that forms of Amen-Rê, as one might have expected, were the most influential in this place and period, with Mut and Khons as the runners-up. More surprising is the fact that, whereas Osiris occurs only once in the whole series, Isis is often depicted. Another surprise is the complete absence of Seth, a god who was specially honoured by Ramesses II. It was in other places, it seems, that this king honoured Seth—in the Eastern Delta and at Ombos, Tjebu, and Spermeru, as Yoyotte shows in *Bulletin de la Société française d'Égyptologie*, 1950, no. 3, 17-22. Here, then, is a *caveat* which M. Christophe has probably heeded from the start: these texts will not themselves provide a complete picture of the religious policies of the era, but taken in conjunction with the evidence from other cult-centres favoured by the kings in question, they may well prove revealing.

J. GWYN GRIFFITHS

Literary Fragments in the Hieratic Script. By RICARDO A. CAMINOS. Oxford, 1956. Pp. 71, pls. 30+29. 63s.

This splendid volume adds significantly to our knowledge of Middle Egyptian literature by presenting three new stories and new fragments of known manuscripts of familiar works. On groundwork done by Gardiner, Ibscher, Černý, and Barns, Caminos has sought to extract the maximum of information from these sadly battered documents. At present, probably only further discoveries could add materially to his treatment.

The papyrus fragments, originally bought at Luxor by Golénischeff, and retained in Berlin and England several years for sorting, restoration, and study, belong to the Pushkin Museum of Fine Arts, Moscow. On palaeographical and other grounds, the three new manuscripts appear to date from the end of the Eighteenth Dynasty and were once fine specimens of hieratic calligraphy. Script, spelling, grammar, and new vocabulary are noted in the introduction to each manuscript.

The first new story, *The Pleasures of Fishing and Fowling*, describes the joys of those field sports so familiar from tomb paintings. The extant fragments can nearly all be assigned to three main sections, for which Caminos posits an order unproven but highly plausible and yielding good sense. In the first, honour is paid to the marsh-goddess and other appropriate deities, and the sportsmen become so engrossed in their pleasure that 'no-one thinks of food'. In the second section, amid fishing and a pithy account of snaring wild-fowl in a clap-net, the hero reminisces about 'the good old days' when he had leisure to pursue these sports at will. In the third survive allusions to the gutting of fish, part of a list of good localities for keen sportsmen, and general observations on the sportsman's life. This tale mentions *Sntwy*, father of the god Sobk, for the first time. The word-order of the phrase [*tw-n*] *r sht m hrtw spdw* 'we shall snare birds in thousands' (pl. 1, A2, 6) is remarkable, perhaps emphatic. *Sfyt* (pl. 2, B4, 7) is certainly obscure; with extreme diffidence I venture to query whether a meaning 'disturbance, be disturbed' would not fit here as well as in *Ptahhotep*—i.e. the water is disturbed by the alighting of a number of wildfowl, especially as there are evidently enough birds to snare in the net a few moments later. The surviving portions of this story indicate a crisp, descriptive narrative glowing with enthusiasm for its subject, and new copies are much to be hoped for.

The Sporting King is cast in quite another mould. Words not deeds predominate here. Again the order of the five known sections (A to E) is quite uncertain; Caminos's order of A, then B and C, is quite likely. The tale perhaps opens with the courtiers trying to persuade their king, apparently Ammenemes II, to rest awhile from his burdensome duties, and one *Sehetepibre'ankh*(?) is summoned to coax the king with his eloquence and marsh-lore. He arouses the monarch's curiosity with a cryptic remark, 'I have seen it', and when questioned burgeons forth with a plethora of similes. The king is won over, for then the whole royal family enjoy an outing in the marshes. However, at every juncture the gallant *Sehetepibre'ankh* launches into long declamations whose wild extravagance easily rivals anything from the Eloquent Peasant's lips. Some mythological allusions will doubtless prove useful. The double-barrelled divine name '*Hr-Sdmy*', (personified) 'Sight-and-Hearing', finds parallels far beyond Egypt. In the Ugaritic texts we find among others *Ktr-w-Hss*, artificer-god like Ptah, '*atrt-w-rhm*(y)', a form of the goddess 'Asherah, and perhaps *Gpn-w-Ugr*, the messenger(s) 'Vineyard-and-Field': cf. C. H. Gordon, *Ugaritic Manual*, p. 51, § 8. 54, who also cites, n. 2, YHWH 'Elohim as a possible further example of this kind of name.

The *Mythological Story* counts among its characters the uraeus-goddess Nesret, the 'Lord-of-All', Thoth, the 'Eye' (*Iret*), Horus lord of Mesen, and probably Rē and Osiris (Caminos, p. 45). The surviving scraps clearly indicate a running narrative liberally laced with dialogue, but no connected story emerges at present. A goddess is thirsty, various deities travel and meet, some people are brought before a god against their will—such tantalizing allusions may perhaps indicate a work of the same general class as *Horus and Seth* or *Astarte and the Sea*.

The two sporting pieces throw some additional light on fishing and fowling for pleasure in the Middle Kingdom. So flowery are *Sehetepibre'ankh*'s speeches that one is tempted to ask whether it is just possible that the author was deliberately burlesquing the rhetorical style! These texts also provide the earliest occurrences attested so far of some words hitherto only met in texts of later date and occasionally only in the Graeco-Roman period (so *hdtw*, waterfowl, Caminos pp. 35-36).

The additions to known manuscripts of *Sinuhe*, *Ptahhotep*, and *Merikarē* are useful; so also is the hieratic of the Golénischeff text of *Sinuhe* given in facsimile for the first time. This copy along with five small new fragments makes possible slight corrections to the transcript in Blackman, *Middle-Egyptian Stories*; most are quite small and only their locations will be listed here—one or two alone are worth particularizing.

Gol. MS., p. 1, 1 = *M*(iddle)-*E*(gyptian) *S*(tories), 1 (tiny traces of *m* over *r* of [*s*]mr; *sib*, large, straddles the seated-man det.; insert \sum before $\frac{\text{xx}}$ in *rd*); 1, 3 = *MES*, 2 (new fragment gives *r-t-t* and *p-r* of *hryt-prt*); 1, 6 = *MES*, 3 (tiny trace of *h* in *ibh*); 1, 7 = *MES*, 4 (here, [*ist r*] *f sb-n hm-f*, *c-w-s* should be rubricized); 1, 8 = *MES*, 4 (tiny traces of det. of *Tmhi/w*); 1, 9 = *MES*, 6 (tiny trace of upright *-s* before *smrw*; on *MES*, 6a, n. 13a, cf. Caminos, pl. 24a, n. 1); 1, 12 = *MES*, 7 (for det. after *h:bw*, Caminos reads seated-man

for Blackman pair-of-legs—either theoretically possible, Caminos perhaps better); 1, 15 = MES, 8 (new fragment adds [w]l *imy-tu* before *bity*); 1, 16 = MES, 9 (Caminos, p. 52, here indicates reading *nm* [k'] as in Ashmolean Ostrakon, rt. 11).

Gol. MS., p. 2, 1-2 = MES, 9, G. '18' (the '18' is misplaced at [sm]), and is to be replaced between *wr*[f] and [n]-i of which traces occur on a new fragment, preceded by [snfr]w); 2, 2 = MES, 10, ll. '18' and '19' (the '19' is misplaced; in '18' a new fragment gives lower parts of -i of *hp-n-i* and of seated-man and stroke det. of s, 'man', plus a tiny trace of the *chr*; at line-end a new fragment adds *snd-n-i* which, followed by *n-f*, agrees with Ashm. Ostr., rt. 13); 2, 3 = MES, 10, l. '19' (one new frag. adds -i of *sh-n-i* and also *dmi n*; another adds [w]sh *nn hm* in red); 2, 4 = MES, 11 (the '21' is misplaced; add now uraeus-det. and verse-point for [dsr]); 2, 6 = MES, 12, l. '22' (new frag. adds [m] *snd*); 2, 7 = MES, 12, l. '23' (tiny trace of *sm*-sign in red).

Gol. MS., p. 3, 2+x = MES, 20, l. '43' (read *sh-m-ib* with Caminos, p. 52); 3, 4+x = MES, 20, l. '45' (add now *pdtwt hr-hst* [f]). In MES, 21, page-line 6, alter notation 'end of second page' to read 'end of third page' and correspondingly 'third' to 'fourth' page in note 6a.

To *Merikare* six new fragments now accrue, belonging to the Moscow copy of this text; some readings add slightly to the text (e.g. *m bkw*, a gap in P. Petersb. 1116A, 86) or to its interpretation (e.g. *wnmt-sn nbt* as in P. Carlsberg VI, 2, 6).

For *Ptahhotep* broken sections of much of p. 6, especially in the upper half, of Pap. B.M. 10509 become available. A large number of 'verses' 328 to 413 (Dévaud's numbering) survive but often broken and not in Dévaud's order. Page 7 now contains the beginnings of sixteen lines and larger parts of the first four, corresponding to some of 'verses' 413 to 497. The four small pieces of p. 8 show parts of a dozen more 'verses'. Scholars will be able to pencil-in the new additions in the blank spaces in brackets left in Z. Žába's new edition of *Ptahhotep*.

This being an Oxford book, misprints are very rare; two are *dd-ti* for *qd-ti* on p. 50, and *c* for *k* in *Merikare* on p. 56^b of the Index. To the industry of Caminos and his predecessors and to the generosity of Sir Alan Gardiner in assuring publication, all Egyptologists are indebted for this new material on Egyptian literature and language, so presented that its great value is not eclipsed by its fragmentary state.

K. A. KITCHEN

Koptische Grammatik (Saïdischer Dialekt) mit Bibliographie, Lesestücken und Wörterverzeichnissen. By WALTER C. TILL. Otto Harrassowitz, Leipzig, 1955. Pp. 360, pls. 2. DM. 38.

It is to be hoped that the successive discoveries of Manichaean and new Gnostic writings in Egypt will have helped to rekindle interest in the Coptic language, which for many years has been somewhat overshadowed by the study of its venerable parent, Ancient Egyptian. In recent years intending Coptic scholars have been well served by the publication of several grammars. Dr. Till's *Koptische Grammatik*, the latest of these, should do much to foster the study of Coptic. Indeed, the new Grammar should prove a most useful *vade mecum* both for the beginner and for the advanced student. It is handy in size—easily slipped into the pocket—and though concise in explanation, it covers each grammatical form and usage adequately. Bearing in mind the needs of beginners, Dr. Till has wisely confined his attention to the Sa'idic dialect. In the opinion of the reviewer comparison with other dialects is only confusing to the beginner. It is far better that a student should acquire a sound knowledge of Sa'idic—after all the most fully documented of the dialects—before attempting to read one or more of the other forms of Coptic. Dr. Till has refrained from giving many comparisons with the old Egyptian forms of Coptic words. His grammar is primarily intended to assist the beginner to learn Coptic, and there is much to be said for teaching intending Egyptologists a sound knowledge of the last form of Egyptian before the study of hieroglyphs is attempted.

As might be expected the necessity for precise and exact illustrations of grammatical rules and forms has meant that the majority of examples have been drawn either from the Bible or from religious works translated from Greek originals. Nevertheless, Dr. Till has not neglected the non-literary sources. In every case he has printed the examples in full and has not been content merely to give references which few students bother to look up even when the texts are available. For the benefit of the advanced student full references

are made to all the published studies on the various grammatical forms and usages as they occur. Within the space of 200 pages Dr. Till has succeeded in covering nearly every aspect of Coptic grammar, a remarkable achievement. Perhaps not everyone will agree with the author in his arrangement of some of the material. For example, paragraphs 241-52, which deal with the various kinds of Sentences, would seem to follow more naturally after paragraph 377. Likewise, the sections dealing with the Preposition and the Adverb might be placed more logically after the section on the Verb rather than before it. This is, however, a matter of personal preference.

The ten pages containing a bibliography should be a very useful introduction to the chief works with which the student of Coptic ought to become familiar. The *Lesestücke* of 58 pages contains a good selection of texts both literary and non-literary. Here some criticism might be raised in regard to the representation of the superlineation in the printing of the texts. The stroke over initial letters, e.g. $\bar{\eta}$, $\bar{\alpha}$ and $\bar{\rho}$ in compound verbs, is correctly placed, but a good manuscript would not write $\rho\bar{\alpha}\bar{\iota}$, $\alpha\bar{\alpha}\bar{\eta}$, $\epsilon\omega\tau\bar{\alpha}$, $\eta\bar{\epsilon}\bar{\lambda}\bar{\lambda}\bar{o}$ for $\rho\bar{\alpha}\bar{\iota}$, $\alpha\bar{\alpha}\bar{\eta}$, $\epsilon\omega\tau\bar{\alpha}$, $\eta\bar{\epsilon}\bar{\lambda}\bar{\lambda}\bar{o}$. It is possible that the particular type employed did not allow for a more exact placing of the stroke. This is an unfortunate development in the reproduction of Coptic texts. Certainly in the case of Steindorff's earlier *Grammar* the stroke was generally represented by the type in its proper position. The extract from St. John's Gospel on page 273, to which no exact reference is given, appears to be a reproduction of Horner's text. If this is so, then it seems a little odd that a student should be presented with a conflated text rather than given an original and consecutive text.

No doubt considerations of economy prevented the inclusion of more than two plates. Nevertheless, it would have been better perhaps to have divided the first plate into two, and to have given an example of one of the earlier uncial hands as well as part of the manuscript already illustrated.

The Coptic-German vocabulary is adequate, the references in brackets to the page in Crum's Dictionary being particularly useful. But in regard to the Greek-German vocabulary, it seems to the reviewer that it is unnecessarily confusing for the beginner to list Greek loan-words in their original Greek form. The many Greek words which had become part of the Coptic language should surely appear primarily in their Coptic form. Steindorff reproduced such words in this manner in the vocabulary in his earlier *Grammar*, and it seems a pity to abandon this practice. Would it not be preferable to list the variant readings of a Greek loan-word in their proper alphabetical order, e.g. $\alpha\iota\tau\epsilon\iota$, $\alpha\iota\tau\iota$, $\epsilon\tau\epsilon\iota$, $\eta\tau\epsilon\iota$, &c., rather than be content with an entry $\alpha\iota\tau\epsilon\iota\nu$ followed by the variant Coptic forms?

The full index of subjects treated in the text of the *Grammar* which concludes the book should increase its value and usefulness. It is the lack of such an index which reduces the usefulness of Steindorff's *Lehrbuch der koptischen Grammatik*.

Dr. Till is to be congratulated on the production of this *Grammar*, which should in no small measure further the study of Coptic. The student may be assured that in using this book he has the benefit of a reliable and up-to-date guide to the acquiring of a knowledge of the Coptic language. J. M. PLUMLEY

The Egyptian Coffin Texts, Vol. VI. By A. DE BUCK. University of Chicago Press (English Agents, Cambridge University Press), Chicago, 1957. Pp. xv+415. £5. 12s. 6d.

Professor de Buck and the Oriental Institute of Chicago University are to be congratulated on producing the sixth volume of *Coffin Texts*; a very considerable range of these important religious documents of the Middle Kingdom has now been made available to students, and we hope that the series may be carried to completion. The present volume lives fully up to the standard set by its predecessors, and is in fact very slightly the longest of the series. Of the spells included, a fairly large proportion recurs in the *Book of the Dead*, but as usual the *Pyramid Texts* are less well represented. One *Pyramid* spell which is found again in this volume, however, particularly interests me, namely *Coffin Text* Spell 573 corresponding to *Pyr.* §§ 393 ff., the so-called 'Cannibal Hymn', which was the subject of my first contribution to this *Journal*. A comparison of the later text with the earlier version shows only too clearly why many of the *Coffin Texts* are hard to translate. If in this case we had not the earlier text to help us, difficult though it is, it would have been impossible to arrive at its true measure from the later one; it is quite obvious that at some time between the

Sixth and Eleventh Dynasties a manuscript of this hymn must have come into the hands of a compiler of funerary texts who failed to make head or tail of it and re-edited it according to his own ideas, and it is this bungled version which has been used in the M.K. coffins. It is perhaps not without reason that the scribe of Coffin B4c stopped short at the end of the fourth sentence. He probably found the rest of the text quite unintelligible and wisely left it alone; as it is, his short extract is already seriously garbled and clearly descends from a version different from and apparently even worse than that of the parallel texts S1c and S2c. Since the originals of the majority of the spells of the Coffin Texts are not preserved to us, we are not able to judge the extent of the mangling of texts which may have gone on, but the specimen in question, where we do possess the original, does not inspire confidence in the early M.K. copyists of the more obscure spells.

R. O. FAULKNER

Egyptian Grammar. By SIR ALAN GARDINER. Third edition, revised. Oxford University Press on behalf of the Griffith Institute, Ashmolean Museum, Oxford, 1957. Pp. xxxvi+646, pls. 2. £3. 3s.

Sir Alan Gardiner is to be congratulated on having written an Egyptological best-seller, for, having sold two editions of his *Grammar* right out, he has been impelled to publish a third in a matter of thirty years. This means that the copies of his book in being must be numbered in thousands, with a demand which is still unsatisfied. Even allowing for the fact that libraries and scholars may many of them possess now three or more copies, such a demand for a grammar of a dead African language must surely constitute a record, and is a measure both of the author's achievement and of the abiding interest in Ancient Egypt. In the present edition a certain number of pages have been revised, but the majority of alterations have been inserted under the heading Additions and Corrections. It has thus been possible to avoid extensive reprinting and so to keep the price down to a level which is very low considering the size of the volume and the present costs of book-production, so that this work, essential to the budding Egyptologist, should be within the reach of even the impecunious undergraduate of today.

R. O. FAULKNER



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